Horatio R. Storer

PHYSIOLOGICAL

ANATOMICAL AND PATHOLOGICAL

RESEARCHES.

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PHYSIOLOGICAL

ANATOMICAL AND PATHOLOGICAL

RESEARCHES.

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PREFACE.

The Papers printed in this Volume have, with the exception of two, been published in periodical journals within the last thirteen years. As in several of them, especially in those containing the results of experimental researches on different physiological questions, I took occasion to give a *resumé* of the labours of those who had previously investigated the same subjects, I have added in the form of appendices, placed at the end of the Papers to which they refer, an account of the results arrived at by those who have subsequently been labouring in the same field, and have pointed out in how far they agree with or differ from those I obtained. These appendices are included within brackets, to distinguish them from the original Papers; and one of them is of considerable length, occupying thirty-seven pages. In reprinting these Papers I have not scrupled to make verbal corrections where it was thought advisable,

but special care has been taken not to alter the meaning of any passage, either by introducing new words, or by withdrawing the old, so as to change the character of the statements and opinions they originally contained. This volume does not include all the Papers published by me on the subjects embraced within its title. Those omitted are one entitled "Anatomical Observations," in the Edinburgh Medical and Surgical Journal, No. 128, several cases of Aneurism and Diaphragmatic Hernia, in Nos. 142 and 144 of the same Journal, and some articles in the Cyclopædia of Anatomy and Physiology. Two Reviews, selected from several I have written, have been reprinted for reasons annexed to them. The state of my health has prevented me from bestowing so much attention to the preparation of the appendices affixed to many of these Papers, as I should have wished.

J. R.

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ON THE

RELATION BETWEEN MUSCULAR CONTRACTILITY AND THE NERVOUS SYSTEM.[1]

(FROM THE EDINBURGH MONTHLY JOURNAL OF MEDICAL SCIENCE, MAY 1841.)

MANY physiologists have devoted much labour to ascertain the exact relation of the nervous system to the contractile tissues, and more especially to the muscular. From the universal distribution of the nervous filaments in the contractile tissues of man and of the higher animals—from the facility with which contractions can be induced in the muscular bundles of the limbs and trunk, by causes acting through the nervous system—and from the circumstance that sensation is, in the normal state of the body, linked with many muscular movements—conjoined perhaps with certain theoretical notions regarding the supremacy of the nervous system in the vital actions of the body, the majority of physiologists still maintain that the property of contractility is derived from the nervous system.

An opposite doctrine was advocated by Haller and his followers. From numerous experiments, in which Haller found the contractility of the muscles remain after their nerves had been cut through, and even after they had been

[1] An epitome of this paper was read at the Meeting of the British Scientific Association in 1840.

removed from the body; strengthened by the arguments suggested by the structure and endowments of the lower organised bodies, and by an examination of the order in which the different parts of the fœtus are developed, conjoined with various other facts and considerations, he arrived at the conclusion that the property of contractility is independent of the nervous system, and is inherent in the muscular fibre itself.[1]

This doctrine, directly opposed to that of the "animists," who had become an influential sect at that period, could not possibly be allowed to pass without animadversion; and accordingly a controversy arose among the learned physiologists of Europe, which for several years was carried on with great keenness and acrimony, and has since that time ranged physiologists into two classes—the Hallerians, or those who believe in the inherent contractility or irritability of the muscular fibre—and the Neurologists, or those who maintain that the muscles derive this property from the nervous system. Some of the followers of both sects, but more particularly the Neurologists, have admitted considerable modifications in the tenets so dogmatically espoused by the original supporters of these doctrines. On the other hand, many of the Hallerians have extended the term contractility, and in our opinion justly, to certain contractile movements which may be induced in the arteries, in various secretory ducts, in the cellular tissue, and in the skin. They have also freely admitted certain effects of the nervous system upon the muscular contractility—such as the influence of mental emotions, &c., upon the contraction of the heart, a conclusion which Haller would have willingly avoided. On the other hand, very many Neurologists have given up the original opinion, that muscles derive their contractility

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[1] Mémoires sur les Parties Sensibles et Irritables. Lausanne, 1760. Elementa Physiologiæ, tom. iv., lib. xi. Lausannæ, 1766.

from the central organs of the nervous system—the brain and spinal marrow—and maintain that the nerves distributed in the different muscles have themselves the faculty of furnishing the conditions upon which contractility depends, believing that in the muscles of involuntary motion this is effected by the ganglionic or sympathetic system of nerves, and in the muscles of voluntary motion by the cerebro-spinal nerves. Some, again, exclude the cerebro-spinal nerves from all participation in this faculty, and confer it entirely upon the ganglionic system.

It is not our intention to enter into any lengthened examination of the various arguments which have been adduced by the supporters of these different views, but shall confine ourselves to one point only, on which the opponents of the Hallerian doctrine have more lately assailed it by new facts, against which its founder has left no adequate defence. The point to which we refer is the effect of the injury of a nerve upon the contractility of the muscular bundles in which it is distributed; and on this, the arguments of those Neurologists who maintain that the property of contractility is dependent upon some influence transmitted along the nerves to the muscles, from the central organs of the nervous system, may be thus stated in general terms—that functional and structural derangements of the brain and spinal cord usually produce paralysis of some part of the body, which, if continued for any length of time, is followed by diminished contractility, size, and strength of the muscles paralyzed; and that the section of a nerve, or even the tight application of a ligature around it, most certainly induces those effects in the muscles in which it is distributed. To these arguments it may also be replied in general terms, that all that can be meant by the term paralysis, in the cases referred to, is, that the muscles have ceased to act in obedience to the mental act of volition; for, if mechanical excitation be applied to the nerve, below the point where it

has been cut across or included in the ligature, within a certain period of time after the muscles have been thus rendered quiescent, equally active and durable contractions can be produced in the muscles said to be paralyzed, as in other muscles whose nervous connexion with the central organs of the nervous system has been left untouched; and that the subsequent disappearance of the contractility in muscles thus insulated from the central organs of the nervous system, is due to the imperfect nutrition that follows a state of inaction, in whatever organ of the body this may be induced.—We shall now examine the specific facts upon which these arguments are based, and attempt to ascertain their relative value. Valli[1] observed that the muscles of the limb of a frog frequently ceased sooner to respond to the excitation of galvanism, when the large nerve passing to them had been left uninjured, than when it had previously been divided. Dr. Fowler[2] found that when one sciatic nerve was cut in a frog nine days before decapitation, no apparent difference could be detected in the vigour and persistence of the contractility of the two posterior extremities. Nysten[3] satisfied himself upon the bodies of two apoplectic patients, who died several days after being attacked with the disease—the one from the first and the other from the second attack—that the application of galvanism excited as violent contractions in the muscles of the paralyzed as of the sound side. Dr. Wilson Philip performed and repeated the following experiment:—The large nerve of the posterior extremity of a frog was divided, and the limb wholly deprived of sensation and voluntary movements. The skin was then removed,

[1] Experiments on Animal Electricity, &c., p. 21. London, 1793.

[2] Experiments and Observations on Animal Electricity, p. 109. Edinburgh, 1793.

[3] Recherches de Physiologie et de Chimie Pathologique, p. 369. Paris, 1811.

and a stimulant (a solution of salt) was kept constantly applied to the muscles till no further contraction could be excited in them, which happened in about twelve minutes. The skin was then removed from the opposite limb, and the muscles subjected to a similar treatment, without injuring the trunk of the nerve, and all signs of contractility ceased in a somewhat shorter period than in the limb in which the communication between the muscles and the central organs of the nervous system had been cut off.[1] This difference may be accounted for by the circumstance, that in the limb in which the nerve had been left entire, the animal would continue to exercise voluntary movements, so that its muscles were exposed both to the effects of the artificial stimulants and of the nervous agency. Mr. J. W. Earle,[2] after testing the facts advanced by Dr. W. Philip, admits their accuracy, but objects to the conclusions deduced from them, and maintains that they can only enable us to judge of the amount of contractility in the muscles at the time the experiment was commenced, and cannot give us any information as to the source whence it was derived; and has further maintained, that before we can agree in the inference drawn from such experiments, it would be necessary to ascertain if the contractility would return with equal rapidity in the limbs, after having been treated in the manner described. These objections, it is obvious, apply equally to the experiments of Nysten, Valli, and Fowler, as to those of Dr. Wilson Philip. Mr. Earle instituted a few experiments on this point, and states that he has satisfied himself, that while the contractility returned in the limb in which the nerves had been left entire, it remained extinguished in that in which the nerves had been divided. The mode of experiment followed by Mr. Earle

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[1] On the Vital Functions. Exper. 35.

[2] A New Exposition of the Functions of the Nerves. 1833.

is, however, liable to an obvious source of fallacy, for it must necessarily have produced great inflammation and all its consequences in the limbs thus treated; and from Mr. Earle's own description, it would appear that the limb in which the nerve had been cut was more disorganized than that in which it had been left entire. If such an experiment could be relied upon, it would most decidedly prove that, after the muscles of a limb had been cut off from all nervous communication with the central organs of the nervous system, and then exhausted of their contractility, they never regain this property. With the view of deciding this point, we made, at the suggestion of Dr. Alison, an extensive series of experiments, the results of which were laid before the British Scientific Association.[1] These were made also upon frogs, and in such a manner as to exhaust the muscles of their contractility without disorganizing the textures of the limb. The large nerve of one limb was exposed, cut across, and part of it removed, while in the opposite limb it was simply exposed. The skin was allowed to cover the muscles in both limbs. The muscles of both limbs were then strongly galvanized until they had ceased to contract—one wire being applied to the nerve, the other being placed on different parts of the surface of the limb, and a solution of salt used to moisten the part of the surface of the limb upon which the wire was applied. It was ascertained that the contractility returned as quickly and strongly in the one limb as in the other, though in one of these the muscles had been insulated from all nervous connexion with the brain and spinal cord, as evinced by the circumstance that they remained quiescent when the cord was crushed and a stilet pushed down the spinal canal, while the muscles of the other limbs were thrown into violent contractions.

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[1] Transactions of British Scientific Association, vol. iv., p. 671. 1835.

We have, in the result of these experiments, sufficient evidence that the contractility will reappear in the muscles of a limb in which it had been previously exhausted, and when the nervous connexion between them and the central organs of the nervous system had been broken through; and are, consequently, forced to conclude that this property of contractility cannot be derived from the central organs of the nervous system. We have also several times performed the experiment by using pure water to moisten the limb, instead of the solution of salt, and with the same result. No one can even perform the experiment of exhausting the muscular contractility in the limb of a frog, after the sciatic nerve has been divided, without feeling fully satisfied that the property of contractility cannot be derived from the central organs of the nervous system; for he finds that if he again applies the wire a very few minutes after the contractility of the muscles has ceased to respond to the excitation of the galvanism, pretty strong contractions may again be produced, and this rapid reappearance of the contractility will occur many times in quick succession. In performing such experiments, it is found to be a tedious process to exhaust the contractility; and even when we have succeeded most perfectly in our endeavours to exhaust it, feeble muscular contractions may be excited after a quiescence of a few minutes. Dr. Marshall Hall has suggested, that in these experiments the division of the nerve may have acted in the manner of a shock upon the muscles in which it is distributed; and the reappearance of vigorous muscular contractions after from two to four days, may be due to the passing off of the effects of the shock, and not to the muscular bundles having regained that contractility which was lost.[1] With the view of testing that sug-

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[1] Cyclopædia of Anatomy and Physiology—Article *Irritability*, vol. iii.

gestion, I exhausted the muscles of the posterior extremity of a frog, having removed a portion of the sciatic nerve, as in the former experiments. After waiting four days, the muscles of the limb were again exhausted, but the contractility, two days after this, had returned as vigorous as before. The experiment was continued further, and these muscles were exhausted four different times, and each time the contractility returned, though the trunk of the nerve had not been disturbed after the first part of the experiment, when a portion of it was removed. This experiment was repeated, and with the same results when the animals were healthy. These facts are sufficient to prove, apart from other considerations, that the reappearance of the muscular contractility in these experiments cannot be attributed to any supposed temporary diminution of it, in consequence of the injury done in dividing the nerve supplying the muscles. Müller has detailed some experiments which he believes to be subversive of the Hallerian doctrine, and to which he appears to attach much importance. He divided the ischiatic nerve in the middle of the thigh in a rabbit; and though, after the expiration of a period of one month and twenty days, the muscles of the limb still contracted when irritated, yet if a longer period was allowed to elapse, they were found to have lost their contractility. In an experiment upon a rabbit, five weeks after the division of the nerve, contraction of the muscles could not be excited by irritating the nerve either mechanically, by a chemical stimulus, caustic potash, by galvanism, or by irritating the muscle itself. The muscles in the limb of a dog contracted slightly ten weeks after the division of the nerve leading to them.[1] Similar results had been observed by Fowler in experiments upon frogs.[2] But before we can admit that

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[1] Elements of Physiology, by Baly, pp. 460 and 680. 2d Edition.
[2] Opus cit.

these experiments afford any evidence in favour of the opinion that muscular contractility is dependent upon some influence transmitted along the nerves to the muscles by the central organs of the nervous system, it would be necessary to prove that a state of complete inaction for so long a time would not of itself be sufficient to produce such effects, since we know that muscles rapidly lose in bulk and in the vigour of their contractions, when thrown into a state of perfect quiescence. And this inquiry becomes the more necessary, when we are informed by Valentin, that on microscopic examination of the muscular fibre after such experiments, the diminution in the vigour of the muscular contraction was proportionate to the physical changes which had taken place in the structure of the muscular fibre, as observed through the microscope;[1] and we also learn from Mr. Skey that he has observed similar changes in the muscles of the human species, thrown for a long time together into a state of inaction, and where the nervous communication between the muscles and spinal cord had not been interrupted.[2] With the view of obtaining satisfactory evidence on this point, the following experiments were performed:—

Exper. 1.—The sciatic nerve was divided in a rabbit, and a portion of it removed. One wire from two galvanic batteries of thirty pairs of plates, was applied over the course of the nerve, and the other wire was applied over the foot, which was kept moist, until the muscles had ceased to contract. Three days after this a weaker battery was used, and the muscles of the limb had recovered their contractility, and contracted powerfully. The more powerful battery was used as before, until the muscles had ceased to respond to

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[1] De Functionibus Nervorum Cerebralium et Nervi Sympatheci, pp. 126–7. Bernæ, 1839.

[2] Philosophical Transactions, (1837,) p. 371, vol. xlviii.

the excitation, and three days after this they had again recovered their contractility.

Exper. 2.—The sciatic nerve was divided in a rabbit, and a portion of it removed. Seven weeks after the operation, the animal was killed by a dose of prussic acid, and the muscles of both posterior extremities were exposed and irritated by the direct application of the wires of a galvanic battery to the muscles. The muscles of the leg of the paralyzed limb contracted very feebly, while those of the other leg were thrown into powerful contraction. The muscles of the leg of the paralyzed limb were evidently much smaller, paler, and softer than the corresponding muscles of the opposite leg. The muscles of the two legs were then carefully removed, and weighed in a delicate balance, and their respective weights were as follows :—

Weight of the muscles of the leg of the sound limb, .	327 grains.
„ „ „ paralyzed, .	170 „
Weight of the tibia and fibula of sound limb, . .	89 „
„ „ „ paralyzed, . .	81 „

On examining portions of the muscles of both legs under the microscope, a very obvious difference in the physical appearance in the muscles of the two limbs at once presented itself. The muscular fibres of the paralyzed leg were considerably smaller, had a somewhat shrivelled appearance, and the longitudinal and transverse striæ were much less distinct than in the muscles of the sound leg. From these experiments, we believe we are justified in concluding—First, That in the warm, equally as in the cold-blooded animals, the contractility will return as vigorously as before in muscles which have been insulated from the central organs of the nervous system, and their contractility exhausted, or at least much enfeebled. Secondly, That the loss of contractility which subsequently occurs in muscles insulated from the central organs of the nervous system,

may be satisfactorily explained by their imperfect nutrition, consequent upon the state of inaction into which they are thrown.

To decide whether or not this imperfect nutrition was dependent upon inaction, or upon any supposed nervous influence flowing along the nerves to the muscles, another series of experiments was performed.

Exper. 3.—The spinal nerves were cut across, as they lie in the lower part of the spinal canal, in four frogs, and both posterior extremities were thus insulated from their nervous connexions with the spinal cord. The muscles of one of the paralyzed limbs were daily exercised by a weak galvanic battery, while the muscles of the other limb were allowed to remain quiescent. This was continued for two months, and at the end of that time, the muscles of the exercised limb retained their original size and firmness, and contracted vigorously, while those of the quiescent limb had shrunk to at least one-half of their former bulk, and presented a marked contrast with those of the exercised limb. The muscles of the quiescent limb still retained their contractility, even at the end of two months; but there can be little doubt that, from the imperfect nutrition of the muscles, and the progressing changes in their physical structure, this would in no long time have disappeared, had circumstances permitted me to prolong the experiment.[1]

We believe, then, that we have adduced sufficient evidence to shew, that though the facts detailed by Müller are perfectly correct, yet the inferences which he has drawn from them, regarding the dependence of the property of muscular contractility upon the central organs of the nervous system, are untenable. Dr. Marshall Hall has lately

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[1] The application of the results of these experiments to the treatment of certain cases of paralysis, is so obvious as to require no illustration.

published some experiments bearing upon this question.[1] He divided the spinal cord immediately below the brachial plexus, in six frogs, and then cut across the sciatic nerve in one of the posterior extremities of each animal. In these animals, then, both the volitional and excito-motory movements were paralyzed in one extremity, while, in the other extremity, the excito-motory movements were retained, as the nervous communication between the muscles of that limb and the spinal cord was left uninjured. He found, after the lapse of a few weeks, that when galvanism was transmitted through the water in which the animals were kept, while the muscles of the limb which still retained their connexion with the spinal cord were thrown into contraction, the muscles of the opposite limb remained quiescent. "This difference in the degree of irritability in the muscular fibre of the two limbs, was observable when these were entirely separated from the rest of the animal." We have no intention of calling in question the facts contained in the Memoir of this distinguished physiologist, nor the very interesting practical deductions which he has based upon them; we only object to the correctness of his inference, that the source of muscular contractility is seated in the spinal cord. Before we can lay down a law in any of the sciences, or, in other words, arrive at a true generalization, it must include all the facts of the case; and, consequently, if the facts we have detailed in the above experiments be correct, the generalization at which Dr. M. Hall has arrived cannot be the true one, for it does not include these facts. To prevent this argument from being retorted upon ourselves, it is incumbent upon us to endeavour to point out how the facts observed by Dr. M. Hall can be included in the generalization for which we are contending, viz. that

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[1] London Medico-Chirurgical Transactions, vol. xxii., p. 191. 1839.

the property of contractility is inherent in the muscular fibre. Two circumstances likely to aid us in doing this have suggested themselves:—1. As the muscles of the limb in which the sciatic nerve had been cut, could only be called into contraction by direct excitation of the muscular fibre, while the muscles of the limb in which the excito-motory movements were retained, could be called into contraction both by direct excitation of the muscular fibre, and also by excitation of the skin, it is obvious that the more vigorous contractions observed in the limb in which the nerve had been left entire, may have been partly dependent upon the galvanism acting as an excitant upon the skin in the one limb and not in the other. That galvanism can act as an excitant upon the skin, and produce vigorous excito-motory movements, we have satisfied ourselves by experiment. 2. Though the muscles of the posterior extremity of a frog no longer respond to the motive influence of volition, after the spinal cord has been cut across, yet if the excito-motory movements remain, these muscles, as we have satisfied ourselves by experiment, may be occasionally thrown into contraction by various causes, such as the rubbing of the skin against the surface of the vessel in which the animal is placed, when it crawls onwards by the action of its anterior extremities, while those of the limb in which the nerve has been cut remain perfectly quiescent. And if this be the case, it is obvious that these occasional muscular contractions in the one limb, may retard, or perhaps arrest, in the muscular bundles of that limb those physical changes which are incompatible with the manifestation of the property of muscular contractility.

We believe that the facts and experiments we have here detailed are sufficient to prove that the property of muscular contractility is not derived from the central organs of the nervous system. We are also satisfied that a careful consideration of all the facts and arguments adduced in sup-

port of the opinion that the property of contractility is derived from the nervous filaments distributed in the muscular fibres, would prove that they have totally failed in establishing it, and that here the evidence likewise preponderates considerably in favour of the Hallerian doctrine. Upon the examination of this part of the question, however, we do not intend, nor do we think it necessary, to enter.

[Since the preceding remarks on the connexion of muscular contractility with the nervous system were written, several additional experiments and observations on this subject have been published, all of which go to support the facts and deductions herein set forth, and seem to place the Hallerian doctrine beyond the reach of assault. Mr. Bowman,[1] in his researches into the minute structure and movements of the voluntary muscles, has repeatedly seen contractions in separate fasiculi of muscle under the microscope, after they had been entirely isolated from the nervous and vascular tissues, and he maintains, "that these observations prove the property of contractility to be inherent in the very structure of muscle, and to be capable of being called into action without the *immediate* instrumentality of nerves." Stannius,[2] in experiments upon frogs, and Longet,[3] in experiments upon dogs and rabbits, ascertained that when the nerves going to the muscles of a limb are cut, and the animals allowed to live, excitants applied to the nerves beyond the point of division, fail to produce muscular contractions long before they cease to do so when applied directly to the muscles themselves. These last experiments accord with some made many years ago by Nysten,

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[1] Philosophical Transactions for 1840. Part II., p. 491.
[2] Froriep's Neue Notizen, No. 418.
[3] L'Examinateur Médical, Décembre 1841.

upon animals immediately after death, and which, like some similar experiments, were not noticed in the above memoir, because we did not think it necessary to enter upon the discussion of the opinion that the property of contractility is derived from the nerves distributed in the muscles. Nysten[1] ascertained that in all the vertebrated animals, contractions may be induced by direct stimulation of the muscular fibre, for a long time after the excitability of the nerves leading to these muscles has disappeared. Matteucci[2] states, that in animals killed by prussic acid, or by strong galvanic shocks, a galvanic current applied immediately after to the nerves, excites either very feeble or no contractions in the muscles in which they are distributed, while the same galvanic current applied directly to the muscles causes sufficiently powerful contractions; and he therefore infers that the muscular fibre contracts under the influence of the galvanic current, independent of the nerve. Dr. W. H. Madden[3] had, several years ago, likewise proved, in opposition to the statements of Dr. C. Henry, that after death from poisoning, the nerves lose their excitability before the muscular fibre. Matteucci[4] has also made an experiment, confirmatory, as far as it goes, of one of those I have detailed in the above memoir. He cut the sciatic nerves in the posterior extremities of a frog, and while the muscles of one of the limbs were subjected to contractions, by passing a galvanic current through them two or three times a-day, for ten, fifteen or twenty days, those of the other limb were left in a state of repose; and he found that the former continued to contract on the application of the galvanism, while the latter ceased at last to

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[1] Recherches De Physiologie, &c., pp. 377-9. 1811.
[2] Leçons sur les Phénomènes Physiques des Corps Vivants, p. 239. 1847.
[3] Transactions of British Scientific Association for 1837, p. 106.
[4] Opus cit., pp. 253-4.

do so. Professor R. B. Todd[1] has given the details of several experiments upon the contractility of paralyzed limbs in the human species, and obtained results adverse to the opinion of Dr. Marshall Hall, referred to above, that in paralysis from lesion of the spinal cord and from the brain, this property of contractility is differently affected, and that the muscles derive their contractility from the spinal cord.

It is stated in a note in the above memoir, that the application of some of the experiments I had described, "to the treatment of certain cases of paralysis, is so obvious as to require no illustration;" for if these experiments be correct, it necessarily follows that the muscles of paralyzed limbs will be kept from wasting, and retain their property of contractility when exercised daily by the transmission of a galvanic current through them, and thus time may be given for the nervous system to recover its power of transmitting outwards the motive influence of volition, and subjecting these muscles again to the action of the will, before their property of contractility has been destroyed by the deficient nutrition consequent upon the state of complete inaction into which they would otherwise have been thrown. The effect of even the occasional contractions of muscles thus circumstanced in arresting wasting from deficient nutrition, is well exemplified in a case of Hysteric Paraplegia, mentioned by Dr. Carpenter;[2] and Professor Paget,[3] in speaking of this method of treatment, adds—"in one case in which I could act upon it, the plan was encouraging."]

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[1] London Medico-Chirurgical Transactions, vol. xxx., p. 207. 1847.
[2] Principles of Human Physiology, 3d edit., p. 443.
[3] Medical Gazette for 6th August 1847.

No. II.

ON THE ORDER OF SUCCESSION IN WHICH THE VITAL ACTIONS ARE ARRESTED IN ASPHYXIA.[1]

FROM THE EDINBURGH MEDICAL AND SURGICAL JOURNAL, APRIL 1841.

A KNOWLEDGE of the order of succession in which the vital actions of the body are brought to a stand in Asphyxia, is not only useful in elucidating the exact nature of the function of respiration, and in pointing out rules for our guidance in the direction of certain remedial agents, but it may also be brought to bear in an important manner upon the investigations into the general laws of physiology. The inquiry is one, however, of unusual difficulty, from the intimate manner in which the respiration is associated in the higher animals with the other vital functions, and the rapidity and energy of the actions and reactions of these upon each other. In conducting such experiments, it is not only necessary to watch closely every phenomenon that presents itself, however fleeting it may be, but all the varied concomitant circumstances must also be carefully analyzed, and, if possible, insulated, with the view of ascertaining how far they might have affected the results. Our progress in such

[1] An epitome of this paper was read at the meeting of the British Scientific Association in 1840.

investigations must, therefore, always necessarily be slow, frequently vacillating and uncertain. These difficulties were found so perplexing, that I had several times nearly given up the present inquiry in despair; and it was not without much labour, and repeated failures, that I arrived at what I considered satisfactory results.

The two points in the physiology of asphyxia that have of late years principally attracted attention are, the nature of the impediment to the circulation of the blood through the lungs; and the cause of the arrestment of the sensorial functions. A correct knowledge of the manner in which the vital actions are arrested in asphyxia, is supposed to be included in the true explanations of these two facts. The first of these, viz. the impediment to the free passage of the blood through the vessels of the lungs, and its consequent stagnation in the right side of the heart, and the large vessels leading to that organ, have been attributed to three causes—the cessation of the mechanical movements of the chest; the effects of the venous blood upon the contractility of the heart; and the difficulty of transmitting the venous blood through the capillaries of the lungs, when the chemical changes which go on there between the blood and the atmospheric air have ceased. The opinion, that the blood in death from asphyxia chiefly accumulates in the right side of the heart and the large vessels leading to it, in consequence of the stoppage of the mechanical movements of the chest, was advocated by Haller.[1] He maintained, that when the lungs were distended with air, as in inspiration, that the blood flowed readily and abundantly through the pulmonary vessels; but, on the other hand, when these organs had collapsed, as in expiration, the pulmonary blood-vessels were so compressed, and their angles rendered so

[1] Elementa Physiologiæ, tom. iii., lib. viii., sect. iv.

acute, that they became in a great measure impermeable to the blood sent from the right side of the heart.[1] Goodwyn[2] argued, in opposition to the mathematical calculations and reasonings adduced by Haller, that, when the lungs are diminished in their bulk, and the acuteness of the angles of the blood-vessels changed only to the extent which occurs during expiration, the flow of blood through them cannot be materially obstructed. He also drew additional arguments in favour of this opinion, from the continuance of the circulation through the lungs, when an amount of fluid was present in the chest sufficient to compress the lung to the extent which occurs in expiration, whether this fluid had been effused in the human species from disease, or induced by artificial means in the lower animals.[3] Goodwyn maintained that the cessation of the circulation in asphyxia was chiefly dependent upon the venous blood failing to excite the contractions of the left side of the heart. "When respiration," he says, "is obstructed, the florid colour of the blood is gradually diminished, and the contractions of the left auricle and ventricle soon cease. The cessation of contraction arises from a defect of a stimulating quantity in the blood itself."[4] The views of Goodwyn were attacked a few years after their promulgation by Coleman[5] and Kite.[6] Both these authors adduced the results of various experiments, to prove that the left side of the heart can contract vigorously upon venous blood;[7] and they also both maintained that they had proved experimentally, that when the lungs are kept mechanically distended during the

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[1] Opus cit., tom. iii., pp. 243-257. 1766.
[2] The Connexion of Life with Respiration. London, 1788.
[3] Opus cit., pp. 40-47.
[4] Opus cit., p. 85.
[5] A Dissertation on Suspended Respiration. 1791.
[6] Essays and Observations, &c. on the Submersion of Animals, &c. 1795.
[7] Coleman, Opus cit., p. 118; and Kite, Opus cit., pp. 26, 42, and 44.

process of asphyxia, that the quantity of blood present in the right side after death is not found to preponderate much, if at all, over that contained in the left side.[1] Bichat also furnished abundant evidence to prove that the left side of the heart can contract vigorously upon venous blood. In numerous experiments he found, that when an animal is asphyxiated, black blood at first traverses the lungs to reach the left side of the heart, and may for a short time be projected from a cut artery, with very considerable force; and he further satisfied himself that the contractions of the heart could be renewed, even after they had become quiescent, in different kinds of violent deaths, by injecting venous blood along one of the pulmonary veins towards the left side of the heart.[2] Bichat especially dwelt upon the importance of discriminating between the effects of asphyxia upon the functions of animal and those of organic life; of ascertaining the priority in the suspension of those two great sets of functions, and the influence which they exerted upon each other. He maintained that the heart's action does not cease because the dark blood transmitted to the left side of the heart cannot excite it to contract, but because the dark blood, by being circulated through the coronary arteries in the muscular tissue of the heart, arrests its contractility. This effect of the dark blood upon the contractility of the heart was, however, regarded by Bichat as a part only of a series of phenomena in asphyxia; for he believed that the vitality of all the tissues of the body was equally effected by the circulation of this dark blood, and that the functions of the brain, or the animal functions, were

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[1] Coleman, pp. 107 to 116; Kite, "From these experiments, it is evident that a small quantity of blood can pass through the lungs when they are in a state of perfect expiration," p. 58.

[2] Sur La Vie et La Mort, Seconde Partie, Article sixième, § ii. Paris, 1805.

always arrested before those of organic life.[1] He maintained that the accumulation of the blood in the right side of the heart did not depend upon any mechanical obstruction in the blood-vessels of the lungs, but from various other causes, among which he enumerates the obstacles opposed to the force of the already enfeebled right side of the heart, by the effects of the circulation of dark blood in the bronchial arteries, and the cessation of the excitation of the lungs by the atmospheric air,[2] aided by the circumstance, that the systemic ventricle can more easily overcome the resistance presented by the capillaries of the body generally, than the veins and pulmonic ventricle can overcome that arising from the capillaries of the lungs. Bichat appears to have entertained doubts whether the circulation of the venous blood through the capillaries of the systemic circulation arrested the vitality of the tissues simply by default of excitation, or by exerting some deleterious influence upon it; for, while discussing its effects upon the brain, he thus expresses himself—" Je ne puis dire si c'est négativement ou positivement que s'exerce son influence; tout ce que je sais, c'est que les fonctions du cerveau sont suspendues par elle." Although Bichat failed in giving the correct explanation of the manner in which the vital actions are arrested in asphyxia, yet there can be no doubt that to him we are indebted for having pointed out the true path by which this knowledge was to be attained. Another important advance was made in the elucidation of asphyxia by the experiments of Dr. David Williams of Liverpool and Dr. J. P. Kay. Dr. Williams,[3] in experimenting on this sub-

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[1] Car, d'après ce que nous dirons, l'affoiblissement qu' éprouve alors le cœur n'est qu'un symptôme particulier de cette maladie dans laquelle tous les autres organes sont le siége d'une semblable débilité, p. 204.

[2] " Le défaut de son excitation par l'air vital," p. 214.

[3] On the Cause and the Effects of an Obstruction of the Blood in the Lungs. Edinburgh Medical and Surgical Journal, vol. xix., p. 524. 1823.

ject, found "that when the chest is laid open immediately after the trachea has been tied during the acme of inspiration, the pulmonary veins soon become empty, while the pulmonary artery continues full." From these experiments he inferred, that, in asphyxia, the blood is obstructed in its passage through the lungs, while its circulation through the other tissues of the body continues; and that the obstruction in the lungs "arises from a deprivation of pure atmospheric air." Dr. Kay, from his numerous experiments,[1] has also arrived at the conclusion, that "the circulation is arrested after respiration ceases; because, from the exclusion of oxygen, and the consequent non-arterialization of the blood, the minute pulmonary vessels, which usually convey arterial blood, are then incapable of conveying venous blood, which therefore stagnates in the lungs."[2] Dr. Kay believes that this stagnation of blood in the right side of the heart and pulmonary artery, occurs in consequence of venous blood being incapable of exciting the capillary vessels of arterial blood of the lungs. The experiments of Dr. W. F. Edwards[3] upon frogs, and those of Dr. Kay upon warm-blooded animals, have very distinctly proved, that the circulation of venous blood in the muscular tissue not only does not exert any deleterious influence upon its contractility, but that this property continues to manifest itself considerably longer when venous blood is allowed to circulate through the vessels of that tissue, than when the circulation of the blood has been entirely arrested.

Though the experiments of Drs. Williams and Kay have demonstrated, that in asphyxia the circulation is first brought to a stand by some impediment to its free passage through the lungs, yet we believe that few will feel satisfied, after a

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[1] The Physiology, Pathology, and Treatment of Asphyxia. 1834.

[2] Opus cit., p. 181.

[3] De L'Influence des Agens Physiques sur la Vie, p. 9. 1824.

careful analysis of them, that they enable us to determine whether this impediment results from the cessation of the respiratory movements of the chest, or from the arrestment of the usual chemical changes between the blood and the atmospheric air—a question of considerable importance in general physiology. When we remember the great influence exerted by the respiratory muscular movements upon the force with which the blood is transmitted along its vessels—a fact first well illustrated by Hales,[1] afterwards by Bichat,[2] and latterly in a more definite manner by Magendie[3] and by Poiseuille,[4] a degree of uncertainty must always exist in interpreting phenomena observed in experiments upon asphyxia, where means have not been taken to obtain the extent and value of this influence.

Such a precaution is the more necessary, since it has been ascertained that blood of a dark colour or venous hue passes at first in the usual quantity through the lungs, and is sent with great force and in a full stream from a cut artery; that it is not until the respiratory movements have been considerably impaired, that it begins to stagnate in the lungs; and that after death, considerable quantities of dark blood are frequently obtained from the left side of the heart. Coleman found that the relative quantity of blood in the two sides of the heart, after drowning, varied considerably; sometimes being as 7 to 4, at other times as 5 to 2, or as 12 to 7. So that, at a medium, the proportions of the right one to the left are about $3\frac{2}{8}$ to $1\frac{6}{8}$. After hanging, the medium was found to be as $2\frac{7}{8}$ to $1\frac{4}{8}$.[5]

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[1] Statical Essays, vol. ii, pp. 1 to 33. 1740.

[2] Sur la Vie et la Mort, p. 208. Paris, 1805.

[3] Journal de Physiologie, tom. i. 1821. Leçons sur les Phénomènes Physiques de la Vie.

[4] Journal de Physiologie, tom. viii., p. 272. 1828.

[5] Opus cit., pp. 7 and 18.

Professor Alison, with the view of supplying this defect in the theory of asphyxia, performed several times the following experiment.[1] A rabbit was confined in nitrogen gas until its respiratory movements had become laboured, and insensibility was approaching. The animal was then withdrawn as rapidly as possible from the glass jar in which it had been confined, and the brain was suddenly crushed by a blow with a hammer, and the chest was immediately laid open. The quantity of blood found in the right side of the heart preponderated considerably over that in the left; and as the respiratory movements had not been interrupted until the animal had been deprived of life, and the circulation in a great measure suspended, these experiments are obviously greatly in favour of the opinion, that the accumulation of the blood in and around the right side of the heart is dependent upon the cessation of the chemical changes between the blood and atmospheric air in the lungs, and not upon the arrestment of the mechanical movements of the chest.

It appeared to me that very conclusive evidence might be obtained on this question by a series of experiments performed in the following manner:—A tube with a stop-cock was fixed into an opening in the trachea, and one of Poiseuille's hemadynamometers was introduced into the femoral artery, for the purpose of obtaining definite information upon the force with which the blood was transmitted along the arterial system. The stop-cock of the tube in the trachea was then shut, and when the respiratory process had been suspended sufficiently long to cause a decided fall in the column of mercury supported by the blood sent along the femoral artery, a large bladder full of pure nitrogen, with a brass nozzle provided with a stop-

[1] Edinburgh Medical and Surgical Journal, vol. xlv., p. 103. 1836.

cock, was fixed in the tube in the trachea, which it fitted accurately, and both stop-cocks were then opened. After the effects of the respiration of the nitrogen gas had been ascertained, a bladder of the same size as the other, similarly provided with a nozzle, and full of atmospheric air, was then substituted for the bladder containing the nitrogen, and the results compared. The difference between the effects of the respiration of the nitrogen gas and the atmospheric air was most marked, and of such a nature as could not be mistaken; for while the mercury continued to fall in the instrument during the respiration of the nitrogen gas, it rose very rapidly immediately after the atmospheric air had entered the lungs and acted upon the blood. In this experiment the same mechanical movements of the chest which failed to renew the free circulation of the blood through the lungs when nitrogen gas was inspired, rapidly effected that object when atmospheric air was permitted to enter the lungs, even when tried on the same animal subsequently to the failure of the nitrogen, and, consequently, at a more advanced stage of the process of asphyxia. This experiment was repeated several times, and, when the requisite care was taken to procure and employ pure nitrogen, invariably with the same results.[1]

Before directing the attention of the reader to a table containing the results of one of these experiments, it will be necessary to take notice of a very unexpected phenomenon which presented itself, and for a considerable time completely embarrassed and perplexed me. Before commencing these experiments I conceived from *a priori* reasoning, that when the blood had become dark in the arteries and the animal functions had been suspended,

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[1] In the experiments first performed, the mercury rose in the instrument, but the nitrogen was mixed with a quantity of atmospheric air, as was proved by the blood becoming partially arterialized in an exposed artery.

the mercury would begin to fall gradually and steadily in the hemadynamometer, and that there would in a short time be a marked depression in the level of the mercury. The mercury, however, actually stood higher in the instrument, and the large arteries became more distended and tense for about two minutes after the animal had become insensible, when the blood in an exposed and unobstructed artery was equally dark as that in the accompanying vein and when the attempts at respiration were few and imperfect, than before the stop-cock in the trachea was shut and when the animal was breathing atmospheric air freely. This was so unlooked for—at first sight was so inexplicable, and so much at variance with my preconceived notions on the subject, that I was strongly inclined to believe that there must be some source of fallacy; but after repeating the experiment more than twenty times, and invariably with the same results, I was at last compelled to admit its accuracy. I then began to surmise that this arose from an impediment to the passage of the venous blood through the capillaries of the systemic circulation, similar to that pointed out in the capillaries of the pulmonic circulation, by which the force of the left ventricle was principally concentrated in the arterial system, and on placing a hemadynamometer in the vein of the opposite limb, and comparing its indications with the instrument fixed in the artery, this supposition, as may be seen from the annexed tabular view of the results of one of these experiments, appeared to be verified. This fact may explain how a quantity of blood is retained in the left side of the heart in asphyxia. It was also ascertained that, though the fall of the mercury in the instrument after the animal was nearly asphyxiated took place very gradually at first, it at last fell very rapidly. Suppose, for example, that the mercury in the tube ranged between 4½ and 5 inches in height before the entrance of fresh air into the lungs was prevented, it rose above this

when the animal had ceased to struggle; it afterwards fell very gradually to between 3 and 4 inches; and when it had fallen to between 2 and 3 inches, it frequently sunk very rapidly to the original level. When atmospheric air was allowed to enter the lungs after the mercury had sunk low in the instrument, no sooner had the air acted upon the blood in the lungs than the mercury instantly sprung up several inches, and when the blood had become more perfectly arterialized, it again stood lower, and the range was more limited. The respirations were necessarily much diminished in frequency, also slow and heaving after the stop-cock was opened in an advanced period of the process of asphyxia, and it was remarked, that during each respiratory movement the contractions of the heart were not only performed with increased strength, but likewise with greatly increased frequency. When the animal was breathing freely through the tube in the trachea, was quiescent, and when the blood was fully arterialized, the range of level of the mercury in the tube seldom exceeded half-an-inch, sometimes not so much. When the stop-cock was shut no change took place in the range of the mercury during the first half-minute: generally before the end of the first minute the animal had begun to struggle, and then the range greatly increased—rising during each attempt at expiration, and during the struggling of the animal; falling during each attempt at expiration and during quiescence. In some of the experiments the range of the mercury during these different conditions amounted to about nine inches, and in one experiment to ten inches—making a most material disproportion in the extent of the pressure upon the inner surface of the arterial system of vessels.

TABLE I.

Showing the changes in inches, of the height and range of the mercurial column in the vertical limb of the hemadynamometer in one of the first class of experiments, when the instrument was fixed in the artery only; the intervals of time at which each change occurred, reckoning in half-minutes from the commencement of the operation; with remarks on the state of the animal at these respective changes. The depth and height of the mercury marked at the end of each half-minute indicated, as near as possible, the extent of the range in the level of the column during that interval of time.

Intervals of Time.	Height of Mercury in the tube attached to the artery.		Remarks on the state of the Animal.
Minutes.	Depth.	Height.	
"	"	1.0	When the hemadynamometer was adjusted to artery, the mercury stood at this height in the vertical tube of the instrument.
"	"	5.5	At the instant the stop-cock was turned, it was 5.5.
½	4.0	4.5	Stop-cock on trachea shut. Dog quiet.
1	3.0	7.0	Do. The artery becoming a little dark.
1½	3.0	9.0	Do. The artery black. Animal struggling.
2	2.0	12.0	Do. do. Animal struggling violently.
2½	4.0	9.0	Do. do. Animal quiet.
4½	4.0	8.0	Do. do. do.
5	4.0	8.0	Stop-cock on trachea opened, and a bladder filled with nitrogen gas applied.
6	3.0	6.0	Do. do. do.
"	"	11	Bladder of nitrogen removed, and one filled with atmospheric air applied.
7½	5.0	11.0	Bladder removed. Natural respiration allowed.
8½	5.0'	6	Do. Animal quiet.

TABLE II.

Showing the same conditions in regard to the second class of experiments, in which hemadynamometers were applied to both the artery and vein at the same time.

Intervals of Time.	Height of Mercury in the Tube attached to the Artery.		Height of Mercury in the Tube attached to the Vein.		Remarks on the State of the Animal.
Minutes.	Depth.	Height.	Depth.	Height.	
		0.5		0.0	When the hemadynamometers were adjusted to the vessels the mercury stood at these heights in the two instruments respectively.
		6.0	5.0	6.0	
½	4.0	5.0		4.0	Respiration natural. Dog quiet.
2½	3.5	5.0		4.0	Stop-cock on trachea shut.
3½	3.0	6.0		3.5	Do.
4	2.0	11.0		12.	Do. animal struggling. The mercury thrown over the top of venous tube, which was twelve inches high.
4½	5.5	10.0		12.	Mercury stood at top of venous tube.
5½	5.5	9.0		8.0	Do.
6½	5.0	11.0		3.5	Do.
7½	5.0	8.0		2.5	Do.
8½	2.5			2.1	Do.

In some of the other experiments the difference between the height of mercury in the two instruments, when the blood became venous, was not so marked as in this.

In performing these experiments I derived much valuable assistance from several gentlemen, but more especially from Mr. James Spence and Mr. K. T. Kemp.

We now proceed to examine the explanations which have been given by physiologists of the cause of the arrestment of the sensorial functions in asphyxia. We have already stated that Bichat maintained that the suspension of the

sensorial functions was caused by the circulation of venous blood in the arteries of the brain; while Dr. Kay believes that he has proved that it is principally dependent upon a diminished supply of that fluid being sent along the systemic arteries, in consequence of the impediment to the circulation through the lungs, and not because the blood sent to the brain is venous—an opinion somewhat similar to that maintained by John Hunter.[1] The experiments of Dr. Kay, in which he injected, "gradually and gently,"[2] four drachms of venous blood into one of the four arteries conveying arterial blood to the brain, through a very small syringe, "having a beak with a capillary bore,"[3] though undoubtedly sufficient to prove the highly unsatisfactory nature of the evidence adduced by Bichat in support of his position, that the sensorial functions are arrested by the circulation of venous blood in the arteries of the brain, cannot, however, be adduced as satisfactory evidence against the doctrine itself. Such an experiment may prove that the transmission of a certain quantity of venous blood along *one* carotid artery is not sufficient to produce cerebral derangement; but it cannot enable us to determine what would be the effect of the passage of venous blood along *all the four* arteries of the brain. We have very frequently watched an exposed carotid artery in an animal during the process of asphyxia, and have observed that the blood flowing along it gradually becomes darker and darker; and we were satisfied that considerably more venous blood than in the experiments now referred to is circulated through the brain for a short time before the animal is seized with convulsions and insensibility. It is evident, then, that if the suspension of the sensorial functions is caused by the presence

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[1] Hunter's Works, by Palmer, vol. iv., pp. 168-170.
[2] Opus. cit., p. 194. [3] Opus. cit., p. 193.

of dark blood in the arteries of the brain, it must be circulated in greater quantities, and for a longer time, than occurred in these experiments of Bichat and Dr. Kay. Before we can proceed further in this inquiry, it will be necessary that we examine the variations in the quantity and force with which the blood is sent along the arteries, and returned by the veins during the process of asphyxia. We have already stated that the arterial pressure, as ascertained by the hemadynamometer, is very little altered during the first half-minute after the entrance of fresh air into the lungs has been suspended; that about the end of the first, or the beginning of the second minute, when the animal commences to struggle, the pressure is greatly increased, and that, generally, for about two minutes after the animal had become insensible, and had consequently ceased to struggle, the pressure was even greater than before the commencement of the experiment. It was also repeatedly ascertained, that the venous pressure, as indicated by the hemadynamometer introduced into the jugular and femoral veins, was equally great for a short time after the animal had become insensible as before the respiration had been suspended. When an artery is cut across immediately after insensibility has supervened, the blood springs from it in a full stream, and with a force equal to what would occur if arterial blood was circulating in the vessels. The insensibility in asphyxia cannot therefore depend upon any diminution in the force with which the blood is sent along the arteries of the brain, nor upon any diminution in the vascular pressure upon that organ. As, however, the frequency of the pulsations in the arteries becomes remarkably diminished before the circulation has been fairly suspended, we are naturally led to inquire if any change in the quantity of blood sent along the arteries of the brain could account for the suspension of its functions. With this view we performed several experiments upon dogs. A tube, furnished

with a stop-cock, was introduced into the trachea, and firmly secured there, the femoral artery was then laid bare, that the changes in the blood might be observed and the number of pulsations more carefully reckoned. We shall give the details of four of these experiments :—After the femoral artery had been laid bare, the pulse ranged from 105 to 120 in a minute, and the respirations were very short and rapid. At the end of the first half-minute after the stop-cock was turned, the pulse was 92. At 1½ minute the pulse was about 120; the animal had begun to struggle, and the blood in the artery was decidedly dark. At the second minute the blood in the artery was nearly as dark as in the accompanying vein, but from the struggles of the animal it was impossible to reckon the pulse. At the end of 2½ minutes the animal had ceased to struggle, was evidently insensible, and the pulse was 42. At the beginning of the fourth minute, the pulse was still 42. The stop-cock was now opened, and the animal allowed to breathe. When the blood was becoming red in the artery, the pulse was 78. A short time after this, when the animal was rapidly recovering its consciousness, the pulse was 60, and the respirations about 132. In another experiment the pulse was 80 at the time when the stop-cock was closed. At the end of the first minute the pulse was 114, and the blood was decidedly darker, and the animal was struggling. At the 1½ minute the animal was struggling, and the blood was nearly as black as in the accompanying vein. At the end of 2½ minutes the pulse was 60, irregular in frequency—two beats following each other rapidly; the animal had ceased to struggle, and the blood was as dark as in the vein. At the end of the third minute the pulse was still 60, and irregular. In a third experiment the pulse was 100 before the stop-cock was turned. At the end of one minute the blood was getting dark, the animal had begun to struggle, and the pulse was 120.

During the course of the second minute it struggled violently, and the pulse could not be reckoned. At the end of $2\frac{1}{2}$ minutes the animal had ceased to struggle, the respirations were few and heaving, and the pulse was 78. At the end of the third minute the pulse was 60. In a fourth experiment the pulse ranged from 88 to 96 before the stop-cock was turned. After half-a-minute the pulse was 71, and the blood was somewhat darker. After $2\frac{1}{2}$ minutes the animal had ceased to struggle, the blood was as dark in the artery as in the vein, and the pulse was 70. At the end of the third minute the efforts at breathing had nearly ceased, and the pulse was 66. In such experiments as these, it is impossible to ascertain the exact frequency of the pulse at the precise moment when the sensorial functions are suspended, in consequence of the struggles and convulsive movements with which this is preceded. Taking, however, all the circumstances of the experiments into account, and combining with them the facts ascertained in those previously detailed to prove that the arterial and venous pressure is not diminished at the time that the animal has become insensible, we have little difficulty in arriving at the conclusion, that, although the pulse has become less frequent about the time that the insensibility has supervened, yet this has not taken place to such an extent as to justify the opinion, that the arrestment of the sensorial functions depends upon any diminished transmission of blood through the vessels of the brain. If a diminution in the frequency of the pulse to the extent we have indicated could produce insensibility, this would frequently present itself during the course of disease, and under other circumstances where nothing approaching to it is observed. It must also be remembered that the pulse, as ascertained before the experiment had been commenced, must have been more frequent than usual, from the terror of the animal. In these experiments I regarded the animal as in

a state of insensibility when the struggles and convulsive movements had ceased. The function of respiration continued for a short time after the suspension of the sensorial functions, but rapidly became enfeebled. The circulation of the dark blood in the vessels of the encephalon, therefore, arrests the functions of the cerebral hemispheres before those of the *medulla oblongata.*

Dr. Kay has performed several experiments,[1] from which he has drawn conclusions very different from those we have just stated. He found, that when the abdominal aorta was cut across in a rabbit of the ordinary size, "nearly seven drachms and three-quarters of blood would escape from the divided aorta when respiration was unobstructed." He then proceeded to cut this vessel across at different periods after the admission of fresh air into the lungs was precluded, and found that, when cut across half a minute after this, the blood collected almost equalled what would have escaped if the free access of air into the lungs had been permitted. In another animal it was cut across after a minute and a half, and five drachms of blood escaped; when postponed to two minutes and a half, four drachms were collected; and when delayed to the termination of the third minute, only two drachms were collected. In judging of the value to be attached to these experiments of Dr. Kay, two circumstances are to be taken into account—the time an animal requires to bleed to death, and the precise time at which the sensorial functions are arrested. As there can be no doubt that an impediment to the circulation through the lungs does occur in the course of the process of asphyxia, it is therefore a matter of considerable importance to ascertain not only the precise time at which the sensorial functions are arrested, but also the average

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[1] Opus cit., pp. 185-188.

period of time the blood would continue to flow from a cut artery when the respiration is unobstructed, before we can venture to determine whether there is any relation between the suspension of the sensorial functions and the arrested circulation in the lungs. With the view of satisfying myself on these points, the abdominal aorta in a rabbit breathing naturally was cut across a little above its bifurcation. The blood continued to flow freely for about one minute; it flowed feebly for another minute; and very feebly for about forty seconds more. In this experiment two minutes and forty seconds elapsed before the bleeding from the artery had ceased. Though in some subsequent experiments the hemorrhage had ceased in a somewhat shorter time, yet we believe that in the rabbit it seldom stops before two minutes have elapsed. With regard to the other point we have mentioned, viz., the exact period at which the sensorial functions are arrested, this has been most unaccountably overlooked by Dr. Kay. He seems not to have been aware that a dog generally becomes insensible in from two to two and a half minutes, and a rabbit in one minute and a half after the complete occlusion of air from the lungs, so that experiments such as those he has related, made to ascertain the quantity of blood which flows from a cut artery at periods posterior to the occurrence of the suspension of the sensorial functions, cannot be adduced in explanation of effects that have previously happened. In performing the above experiments, as has already been mentioned, we took the cessation of the struggles and the convulsive movements of the animal as a test of insensibility. When a ligature is tied tightly around the trachea of a rabbit, the animal moves about nimbly at first, but, before one minute and a half have elapsed, it has fallen down in a state of insensibility, and the attempts at respiration are few and heaving. As the manifestation of the functions of the *medulla oblongata*, upon which respiration

depends, are not necessarily linked with that of the functions of the cerebral hemispheres or the sensorial functions, it must be evident that, in attempting to discover the cause of the cessation of the mechanical movements of the chest, the frequency of the respirations ought to be attended to, and not the suspension of the sensorial functions. This circumstance has not been overlooked by us in performing these experiments; and we are satisfied that the function of respiration is much enfeebled at a period of the process of asphyxia, when this cannot be explained by any diminution in the quantity of blood sent to the *medulla oblongata*. No doubt, respiratory movements may be observed after the pulsations have been very considerably diminished in frequency, but these have become few in number, and performed at long intervals before this condition of the circulation has been induced; but it is quite possible that the ultimate cessation of the functions of the *medulla oblongata* may be hastened by the diminished quantity of blood sent along the arteries supplying it. If we proceed, therefore, to analyze the experiments of Dr. Kay, bearing in mind the length of time the blood continues to flow from the divided abdominal aorta of a rabbit, and the precise period at which the sensorial functions are arrested in the process of asphyxia, we must arrive at very different conclusions from those he has deduced from them.

In further confirmation of the views we are advocating, we may appeal to the experience of every practical physician; for he cannot have failed to observe the gradual torpor that frequently creeps over the sensorial functions in severe cases of bronchitis, when an ill-arterialized blood is circulating in the vessels of the brain, and the pulse is still pretty strong at the wrist.

We feel very strongly convinced that Dr. Kay has fallen into another error in stating, that, three minutes after the entrance of air into the lungs had been prevented, the blood

in the arteries had assumed the venous hue "still imperfectly;" for, in numerous experiments, various gentlemen who were present all agreed that the colour of the blood in the arteries was as dark as that contained in the accompanying veins at a period anterior to this. The statement of Bichat, that the blood in the arteries exactly resembles venous blood in a minute and a half or two minutes, is, I am satisfied, much nearer the truth.

From the various facts mentioned above, we have arrived at the conclusion, that the suspension of the functions of the encephalon are chiefly, if not entirely, dependent upon the circulation of venous blood in the arteries. We do not, however, maintain that venous blood exerts any noxious influence upon the functions of the nervous texture; but believe that the effects are solely to be attributed to the want of the proper excitation of the organ; for, when the circulation of arterial blood is renewed, its functions rapidly remanifest themselves, provided that this be done within a given time.

We believe, then, that, in asphyxia, the order of succession in which the vital processes are arrested is as follows:—Dark blood is at first transmitted freely through the lungs, and reaches the left side of the heart, by which it is driven through all the textures of the body. As the blood becomes more venous, its circulation through the vessels of the brain deranges the sensorial functions, and rapidly suspends them, so that the individual becomes unconscious of all external impressions. The functions of the *medulla oblongata* are enfeebled about the same period that the sensorial functions are arrested, but are not fairly suspended for some time longer. Immediately after the sensorial functions are suspended, and the blood has become still more venous, it is transmitted with difficulty through the capillaries of the lungs, and consequently begins to collect in the right side of the heart. A smaller quantity of blood

must now necessarily reach the left side of the heart; and this diminution of the quantity of blood sent along the arteries, conjoined with its venous character, and the ultimate arrestment of the circulation, being circumstances incompatible with the manifestation of vitality in the other tissues of the body, general death is sooner or later induced.

The persistence of the muscular contractility after the arrestment of the circulation, varies, as we have had frequent opportunities of witnessing, according to the age and strength of the individual, and also in a very marked manner from constitutional causes, which are unknown; and in this way we are able to explain how the heart's action may be renewed a considerable time in some cases after apparent death, while in others all the attempts to restore animation, though commenced shortly after the suspension of the sensorial functions, have failed. It must be obvious that the first and principal object in the treatment of asphyxia is to restore the circulation through the lungs. If once we succeed in this, and thus renew the heart's action, the arterial blood is again transmitted to the encephalon and the other tissues of the body; the functions of the *medulla oblongata* remanifest themselves; the sensorial functions are gradually restored; and the animal heat returns. The derangement of the functions of the *medulla oblongata* and the sensorial functions are not necessarily coequal in extent, and never in importance, in asphyxia; and this is well observed in some of those cases of death from disease or narcotic poisons, where the process of asphyxia occurs more slowly and gradually. In these it is not unusual to find the sensorial functions nearly or entirely suspended, at a time when the respiration is pretty effectively carried on; and it is evident, from various facts, that the arrestment of the muscular respiratory movements is not dependent upon the suspension of the sensorial functions, but upon arrestment of the functions of the *medulla oblongata*.

We shall now proceed to make some remarks upon the increased force with which the blood is sent along the arteries during muscular contraction. It has been proved, as we have already mentioned, that the blood is sent with greater velocity and increased force along the arteries during the contraction of the muscles of the limbs and trunk, as in exercise, and this takes place in a more marked manner during violent attempts at expiration. On the other hand, during violent attempts at inspiration, the pulse becomes less frequent, feeble and soft. In some of the experiments we performed, as we have already mentioned, the mercury rose as high as the eleventh, and in one to the twelfth inch of the scale attached to the tube, during violent attempts at expiration and the struggles of the animal, while it fell as low as the second inch during violent attempts at inspiration. During these different conditions the pressure upon the external surface of the heart and its position in the chest must be somewhat altered, a certain amount of pressure being applied to its outer surface during expiration, and removed during inspiration ; and it recedes deeper into the chest during inspiration, and again comes forward during expiration ;[1] but we may safely set those aside as exerting no appreciable influence in the production of the phenomena in question. Müller believes that the increased contractions of the heart accompanying muscular movements of the trunk and limbs may be caused by a sympathetic or reflex action—an excitant effect being produced in the filaments of the nerves distributed in the contracting muscles, which, being conveyed inwards to the spinal cord, is reflected upon the heart. As, however, he adduces no

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[1] In mentioning this fact in the article *Heart*, in the Cyclopedia of Anatomy and Physiology, the word inspiration has been inadvertently printed for expiration, and *vice versa*.

direct evidence in favour of this opinion, we do not feel inclined to abandon the old explanation, that this is merely dependent upon the mechanical acceleration of the blood, by the pressure exerted upon the blood-vessels by the surrounding muscles during their contraction, and the more especially as we have witnessed several facts which at least prove that a great part of the phenomena in question may arise from this cause. We have frequently remarked, that when an animal was breathing very rapidly, even above 100 in a minute, through a tube in the trachea, that the mercury did not rise higher in the instrument than before, and that the range was limited, provided the expirations were always short, and, consequently, not attended with much compression of the blood-vessels in the thorax and abdomen. On the other hand, a marked rise of the mercury took place whenever a forced expiration was made, however slowly this was performed. It was also repeatedly observed, that when one instrument was fixed in the femoral artery, and another in the femoral vein of the opposite limb, the mercury stood considerably higher in the instrument fixed in the vein than in that fixed in the artery, when the animal began to struggle violently. In few of the experiments did the mercury rise much above eleven inches in the instrument in the artery, while it frequently ran over the top of a tube twelve inches high, with considerable force, in the instrument fixed in the vein—showing us in some of these experiments a prodigious increase in the pressure upon the inner surface of the venous system, equal to between three and four pounds on every square inch of surface. This greater elevation of the mercury in the instrument fixed in the vein can only be explained by the effects of the mechanical pressure of the surrounding muscles becoming increased, as the extent of the vascular tubes over which it is exerted becomes elongated, and may afford some indications of the greatly increased impulse communi-

cated to the blood by the powerful pressure exerted by the contraction of the muscles of the chest and abdomen upon their contained blood-vessels, when aided by the contractions of the muscles of the limbs, and favoured by the presence and particular disposition of the valves of those blood-vessels. It is difficult to determine, then, how much this increased flow of blood along the vessels during violent expirations, and during the contraction of the muscles of the limbs, depends upon more forcible contractions of the heart, or upon the mechanical effects of temporary pressure upon the blood-vessels. The increased rapidity and strength of the contractions of the heart during violent expirations must be partly attributed to the compression of the blood-vessels of the lungs, and the transmission of an increased quantity of blood to the left side of the heart, while the diminution in the strength and frequency of the pulse during inspiration must, in a great measure at least, depend upon the sudden removal of that pressure, so that a great part of the blood propelled during a few of the contractions of the right side of the heart immediately succeeding the sudden dilatation of the thorax, goes to fill up the blood-vessels of the lungs to that state of plenitude in which they were before the preceding expiration, and a smaller quantity reaches the left side of the heart.

We do not think it necessary to make any remarks upon the question, whether or not the blood stagnates in the lungs, in consequence of the cessation of the chemical changes between the blood and the atmospheric air, or upon any supposed effect which the venous blood may have upon the contractility of the capillary vessels of the lungs, as this has already been most ably and most satisfactorily done by Dr. Alison. He has shown that this phenomenon is to be referred to an interesting general law in physiology, that has hitherto not received the attention which its importance demands, by which the movement of nutritious

juices is influenced by the chemical changes, or, as he terms them, the vital attractions connected with the chemical changes constantly going on in the capillary vessels between those juices and the surrounding tissues, by which nutrition and secretion are effected. That such a moving power exists, regulating the quantity of blood that flows through each individual organ, independent of any impulse from the living solids, cannot be doubted.[1] Before arterial blood can be transmitted freely through any tissue or organ, it is not only necessary that the contractions of the heart be performed with a certain amount of force, but that the actions of nutrition and secretion be also in operation ; so in the same manner, before the blood can be transmitted through the lungs, it is not only necessary that the right side of the heart retain its contractility, but that the chemical changes between the blood and the atmospheric air should proceed. This doctrine is still further illustrated by the fact which we have ascertained, that, when the blood in the systemic circulation becomes decidedly venous, and unfit for carrying on the process of nutrition, it passes less freely through the capillary arteries into the veins.

[I neglected to mention in the above memoir a modification in the form of Poiseuille's hemadynamometer, used in the experiments there detailed. Finding that, from the nature of the experiments, the hemadynamometers constructed in the usual manner were constantly breaking, I had two made as follows :—The horizontal, descending, and a small part of the ascending portions of the tube of which the instrument is chiefly composed, were formed of brass. The descending portion of the tube was shortened —being only 4½ inches in length—and 1½ inch below the

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[1] Vide Outlines of Physiology, 3d edition, pp. 22-25, 61-64, and 224.

point of its junction with the horizontal portion, it was suddenly dilated into a considerable but short cavity, capable of containing a quantity of mercury sufficient to fill the ascending portion of the tube. Nearly the whole of the ascending portion of the tube was composed of glass, and had, as usual, a scale for measuring the variations in the height of the column of mercury. As this form of the instrument was not only less liable to be broken, but, from the shortening of the descending part of the tube, more conveniently used, and as the results I wished to obtain were chiefly comparative, it seemed sufficiently well adapted for my purposes, and accordingly I employed it in all my later experiments, including all those referred to in the above memoir. It is, however, obvious that the amount of pressure exercised by the blood is to be calculated differently in this form of the instrument, and in that usually employed. In the usual form of the instrument the rise of the mercury in the ascending portion of the tube through any given space, indicates an amount of pressure exercised by the blood equal to twice that extent; suppose, for example, the mercury stood at the height of four inches in the ascending portion of the tube, this would indicate that the pressure of the blood was equal to eight inches—seeing that the forcing up of the column of mercury in the ascending portion is attended by a corresponding depression of the column in the descending portion of the tube. In the instrument used in the above experiments, after the mercury has risen in the ascending tube $1\frac{1}{2}$ inch, the amount of pressure exercised by the blood beyond this may be estimated by reading off from the scale of measurements, the height to which the column of mercury ascends, but without doubling it, as in using the instrument of the usual form. In performing experiments where it is necessary to obtain the absolute amount of the pressure exercised by the blood at different times, the instrument used

in the above experiments would not answer well, and could not properly be substituted for that commonly employed; but as I have already stated, the results I wished to obtain being almost entirely comparative, it suited my purposes sufficiently. When, in the above memoir, the number of inches is mentioned which the mercury rose in the ascending portion of the tube of the instrument, it must be distinctly kept in view that, with the exception of the first $1\frac{1}{2}$ inch, these are not to be doubled to obtain the absolute amount of pressure exerted by the blood; when, for example, it is stated that the mercury stood at the height of 11 inches, the actual pressure exercised by the blood would be a little more than $12\frac{1}{2}$ inches. In the first table given above, it is stated that during the first half-minute after the stop-cock on the tube in the trachea of the animal was shut, the mercury in the hemadynamometer fixed in the femoral ranged between 4 and $4\frac{1}{2}$ inches, while $2\frac{1}{2}$ minutes after, when the animal was quite insensible and all voluntary movements had ceased, it ranged between 4 and 9 inches. Now it is obvious, from the explanation here given, that the greatest pressure exercised by the blood passing along the femoral artery at the latter over the former period, was equal to a column of $4\frac{1}{2}$ inches of mercury.

Since the above memoir was published, Mr. Erichsen[1] has given us the results of an extensive and careful experimental inquiry into the pathology and treatment of asphyxia, and he confirms the accuracy of all my experiments and observations on this subject, as far as he has repeated them. The amount of increase in the force with which the heart drives the blood along the arteries for a short time after an animal becomes insensible in asphyxia, will, I be-

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[1] Edinburgh Medical and Surgical Journal, January 1845, vol. 63.

lieve, be found to correspond nearly in both our experiments, when the difference in the form of the hemadynamometers used is taken into account. But while Mr. Erichsen does not object to my data, he dissents from one part of the theory of asphyxia deduced from them. We agree entirely in our explanation of the cause of the suspension of the sensorial functions—in fact, he has been pleased to say that he does "not feel called upon to make any remarks on this point, as it has already been fully and ably investigated" in the above memoir; but we differ in our explanation of the arrestment of the circulation of the blood through the lungs. I have adopted the opinion that this is due to the cessation of the chemical changes between the blood and atmospheric air in the lungs; while Mr. Erichsen supposes that it depends upon the venous blood acting as an excitant upon the contractility of the ultimate ramifications of the pulmonary veins, and thus causing an obstruction to its passage along these vessels. This view of the cause of the accumulation of the blood in the pulmonic heart and its vessels, adopted by Mr. Erichsen, appears to be incompatible with some well-established facts. The property of contractility possessed by the arteries, is that kind termed by some physiologists *simple contractility*, to distinguish it from the contractility of the muscles of voluntary motion, and manifests itself by slow contractions followed by equally slow relaxations. As Mr. Erichsen himself states, one experimenter found that a period of from one to three minutes elapses before the contractility of an artery responds to excitants. When contraction has been induced in the coats of an artery by an excitant, it is well known that relaxation does *not follow suddenly* even on the withdrawal of the excitant, but it occurs slowly and gradually. If then the ultimate ramifications of the pulmonary veins are stimulated to contraction by the venous blood, relaxation ought *not to follow instantly* upon the withdrawal

of this excitant; and the entrance of pure atmospheric air into the lungs, ought not to be instantly succeeded by the free passage of the blood from the right to the left side of the heart. The following experiment, mentioned by Bichat,[1] and which I have frequently repeated, appears to me to be an *experimentum crucis* upon the point under discussion. If a tube, with a stop-cock upon it, be tied into the trachea of an animal, and the stop-cock turned to exclude fresh air from the lungs until the circulation of dark blood along the arteries has become much enfeebled, as ascertained by exposing a large artery and making an opening into it, *instantly* on the admission of fresh air into the lungs the blood springs from the cut artery, of a bright red colour, and with greatly increased force. I have observed the same thing repeatedly when the hemadynamometer was fixed in the femoral artery; no sooner was the stop-cock opened, and fresh air permitted to enter the lungs, than the mercury suddenly sprung up several inches in the ascending portion of the tube of the instrument. Notwithstanding, therefore, all that Mr. Erichsen has so ingeniously advanced against the part of the theory of asphyxia that refers the impediment to the passage of the blood through the lungs to the cessation of the chemical changes which occur there in natural respiration, my belief in its truth has not been shaken.

We cannot here enter upon any regular discussion of the question to what extent the flow of blood through the capillary vessels of the different textures of the body is influenced by the chemical changes there in constant operation, but we may remark, that all the experiments, similar to those related by Mr. Erichsen, made to prove that a force equal to that exerted by the heart is sufficient to propel the

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[1] Sur la Vie et la Mort, Seconde Partie, article sixième.

blood through the capillaries and along the veins back again to the heart, appear to be rendered nugatory as far as this question is concerned, by various well-ascertained facts in the living body. It is admitted by physiologists, that while the heart drives the blood with equal force to all parts of the body, the quantity of blood flowing through any particular texture or organ is chiefly regulated by certain actions in their capillary vessels. When increased nutrition or secretion occurs in any organ an increased quantity of blood flows through it, and when these functions are diminished in activity, the quantity of blood passing through the organ is proportionally diminished. These different conditions are well exemplified in the mammæ and uterus, and in the annual renewal of the antlers of the stag. The increased flow of blood in these cases cannot be explained by contractile movements of the smaller arteries or capillaries, and it is difficult to understand how the heart is not able to maintain this increased flow of blood after the periods of activity in the functions of nutrition and secretion have ceased, if we look for an explanation of this in any property possessed by the coats of the blood-vessels. Observe the difference between the changes that take place in an artery above the point where it has been secured in a ligature for aneurism, and tied on the face of the stump in amputation. In the former, the collateral branches that spring from the trunk of the artery above the point tied become enlarged, and are able, after a short time, to transmit downwards to the lower part of the limb as much blood as used to pass through the portion of the trunk of the artery now obliterated, while in the latter the collateral branches undergo no such enlargement, in fact, the main trunk, and all its branches for some distance above the point tied, become considerably shrunk. Now, what is the cause of the increased size of the collateral branches in the one case and their shrinking in the other? It cannot be

explained by any addition to the force exerted by the heart, nor by any increase in the contractility of the arteries themselves. Can we explain the shrinking of the collateral branches in the amputated limb? Certainly not by any diminution in the force exerted by the heart, and by no changes in the contractility of the arteries with which we are acquainted. Is it not evident that whatever may be the force exerted by the heart in the circulation of the blood, that there are causes in constant operation that can greatly increase or diminish the quantity of blood flowing through an organ, apart altogether from any changes in the action of the heart, or of any other contractile tissue? Is not the solution of this difficulty only to be found in the view that the chemical changes that attend the operations of nutrition and secretion draw the blood onwards along the vessels leading to the parts where these functions are being performed? It is this *vis a fronte* that regulates in each tissue the supply it receives from the general mass of blood kept in motion by the heart or *vis a tergo*. It is impossible to explain the circulation of the nutritious juices in the vegetable kingdom and in the lower organized animals, without admitting the existence of this *vis a fronte*. In man, and in the greater number of animals, a considerable *vis a tergo*, exercised by the heart, is conjoined with this *vis a fronte* to secure a more abundant and rapid supply of the nutritive juices to the different tissues. We believe that the experiments detailed in the above memoir upon the different effects of the respiration of pure nitrogen gas, and of atmospheric air, upon the pulmonic and systemic circulations, can only be explained by adopting this view; and that they afford the best illustrations we possess in the higher animals, of the extent of the influence of the chemical changes in the capillary vessels in aiding the heart in circulating the blood.

Professor Draper[1] has lately endeavoured to explain, by known chemico-physical laws, these movements of the nutritious juices of plants and animals now under our consideration; and if this explanation be proved to be correct, it will remove one of the chief objections urged against this doctrine, viz., that the same attractive force which is capable of drawing the blood into the tissues will be sufficient to retain them there, and prevent them from passing onwards along the veins. Professor Draper's explanation rests upon the physical law, "that if two liquids communicate with one another in a capillary tube, or in a porous or parenchymatous structure, and have for that tube or structure different chemical affinities, movement will ensue; that liquid that has the most energetic affinity will move with the greatest velocity, and may even drive the other liquid entirely before it." From these premises he proceeds to show, in an apparently satisfactory manner, how the arterial blood should be drawn into the systemic capillaries, so long as the chemical changes between it and the surrounding tissues proceed, and how the venous blood, which has no chemical affinities for these tissues, should be driven onwards along the veins towards the heart. If this occurs in the systemic, the reverse will happen in the pulmonic circulation, for the venous blood has a strong affinity for the oxygen of the atmospheric air that occupies the air cells upon which the pulmonic capillaries are ramified, while the arterial blood has none, and the venous blood is drawn into these capillaries, and drives the arterial blood before it towards the heart. Professor Draper refers to the experiments in the above memoir for a confirmation of the facts which might have been predicted from his theory. Dr. T.

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[1] A Treatise on the Forces which Produce the Organization of Plants, pp. 22 to 41. New York. 1844.

Warton Jones[1] mentions the results of an experiment made by himself, which seem to accord with the explanation of the stagnation of the blood in the lungs in asphyxia given in the above memoir. He observed that when a stream of carbonic acid gas was directed against the lung in the frog, the circulation of the blood is there arrested "by the red corpuscles agglomerating together, and applying themselves here and there flat against the wall of the vessel, and adhering to it."]

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[1] British and Foreign Med. Review, vol. xiv., p. 600. 1842.

No. III.

ON THE EFFECTS OF VENESECTION IN RENEWING AND INCREASING THE HEART'S ACTION, UNDER CERTAIN CIRCUMSTANCES.

(FROM THE EDINBURGH MEDICAL AND SURGICAL JOURNAL, APRIL, 1836.)

I HAVE observed in several experiments on the lower animals, that disgorging the right side of the heart, when its contractions are enfeebled or suspended, by opening the external jugular, has in some cases a decided effect in renewing its action, and this I am convinced may be of considerable practical advantage in promoting the return of the circulation under certain circumstances.

1*st Exper.*—While assisting my friend Mr. Cormack in some experiments upon the physiological effects of creosote, we found, on opening the thorax of a dog, immediately after it had ceased to breathe, into whose femoral vein twenty-five drops of creosote had been injected, that the heart was perfectly quiescent, and remained so even when pricked and cut superficially with a scalpel. As the right side of the heart appeared much engorged, a small opening was made into the auricle, and part of the blood allowed to escape. As soon as the blood began to flow, the heart immediately resumed its contractions, and continued to act vigorously and spontaneously between two and three minutes, and only ceased after five minutes. The same

quantity of creosote was injected into the jugular vein of another dog. On exposing the heart it was perfectly quiescent, and refused to contract when irritated. On emptying the right auricle of part of its blood, it gave two or three contractions, and then ceased.

The poison appeared to have acted more powerfully and rapidly in the last dog than in the first. On applying the hand over the chest of the first dog, an irregular fluttering was felt for a short time after the creosote had passed into the vein, while in the second dog the action of the heart was never felt after it had fairly entered the vein. It is important to remark, that the respiration continued for a short time after the heart's action had become arrested.

Struck with the effect which disgorging the right side of the heart had in renewing its contractions under circumstances where no external stimulus was of any avail, I was anxious to ascertain if the same results would follow the unloading of the heart when arrested from other causes.

2d Exper.—Three dogs were killed by hanging, and as soon as they had ceased to breathe the thorax was laid open. In all of them the heart was acting pretty vigorously, particularly in one only a few months old. When the contractions had become feeble the external jugular vein was opened. This was followed by a decided but temporary increase in the contractions of the heart in two of them, which were large and full-grown. In the third and young dog the effect was very trifling. The opening of the external jugular was found rapidly to empty the right side of the heart.

The marked difference between the results in the first experiment and those that follow, can, I think, easily be explained. It is well known that there are several poisons which produce death by arresting the contractility of the heart, and Mr. Cormack's experiments show that among these creosote must be placed when injected into the veins.

When these poisons are administered in sufficient quantity, they destroy at once and for ever the irritability of the heart, and no remedial agent can be of the slightest avail. We can, however, easily suppose—and facts could be adduced in confirmation of it—that the substance may be given in a quantity merely sufficient to act transiently upon the heart, so as to diminish or arrest its contractility for a short time only; and it would appear that after the action of the poison begins to pass away, and when the contraction of the heart would be renewed, it has, during the temporary suspension of its contractility, become so much engorged with blood, that no external stimulus can excite it to action, until it has been first emptied of part of its blood. The more rapid action of the poison in the second dog, where the renewal of the heart's action was to a trifling extent compared with the first, is in exact accordance with this view. In death from asphyxia the contractility of the heart appears to be but little affected at first, but gradually ceases after the circulation through the lungs has been suspended. And though the right side of the heart, as its contractility diminishes, becomes so engorged with blood that its contractions are suspended, and though opening the jugular vein, if not deferred too long, has generally the effect of relieving the right side of the heart of part of its blood, and renewing its contractions, yet we cannot expect the same decided results as in those cases where the action of a deleterious agent has produced a temporary and fleeting effect upon the contractility of the heart.

3d *Exper.*—While witnessing, along with my friend Dr. J. Y. Simpson, the effects of prussic acid upon some dogs doomed to death at the Police Office, on applying the hand over the chest of a dog, immediately after it had ceased to breathe, to whom a large dose of the acid had been given, the heart was felt beating slowly and irregularly. On opening some of the vessels at the lower part of

the neck, among which was the external jugular vein, the action of the heart instantly became rapid, regular, and stronger.

4*th Exper.*—A rabbit was killed by a large dose of strychnia. When the heart was exposed its contractions were slow and labouring. On opening the jugular vein, the right side of the heart began to disgorge itself, and this was accompanied by a very decided increase in the number and strength of its contractions.

5*th Exper.*—Two rabbits were killed by a blow on the head sufficient to injure a portion of the brain. On exposing the heart, its movements were feeble, and the right side of the heart was engorged. The escape of blood from the external jugular was followed by a decided increase in the number and strength of its contractions, which lasted for a considerable time.

6*th Exper.*—I was now anxious to ascertain what influence artificial respiration, by favouring the passage of the blood through the lungs, would have in renewing or increasing the contractions of the heart in these cases. These experiments (six in number) were performed upon rabbits, and were far from being so satisfactory as I could have wished. We, however, saw sufficient to lead us to believe that though in cases of asphyxia, the artificial inflation of air into the lungs is sufficient, as numerous experimenters have ascertained, to renew the circulation through the lungs, if commenced when the contractions of the heart are still going on pretty vigorously, yet if these are less active, they may be assisted by disgorging the right side of the heart.

I have thought that, instead of prosecuting the subject farther experimentally at present, for the purpose of endeavouring to ascertain the extent of its application to the human species, I should leave it in the hands of those practical men who have ample opportunities of putting it to

the test of experience, if they should think it worthy of their consideration.

In these experiments I was assisted by Dr. Pollexfen, Messrs. Cormack, Skae, and G. Newbigging.

Bleeding from the external jugular vein has been long practised in all cases of asphyxia. Though some may still open this vein in these cases for the purpose of relieving congestion of the brain, it cannot act in this manner, since accurate dissections have shown that no such congestion exists; so that the principal object in bleeding in asphyxia must be to renew the heart's action, and it is with this view alone that some now recommend its use. Though it can have no effect in diminishing the quantity of fluid within the cranium, it may yet assist in removing any irregularity in the distribution of the blood induced there during the disturbed state of the circulation. To produce this effect, the best plan (to judge from the experiments which we have detailed,) is to open the jugular vein and encourage, by all possible means, provided that no air be allowed to pass into the vein, the flow of blood from the lower orifice, and from the lower orifice alone, if we are anxious that much blood should not be drawn. By pressing upon the lower orifice, as is usually done, the blood will be prevented from regurgitating from the right side of the heart, and the most effective method in which blood-letting can favour the renewal of the contractions of the heart will be lost.[1] Conducted in a proper manner, we believe that in many cases it may act as a valuable adjuvant to the artificial respiration, friction with warm flannels, and other remedies that are put in force to restore suspended animation.

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[1] Though the external jugular vein in the human species has pretty generally a valve near its termination, and occasionally another about its middle, yet these rarely present any decided obstacle to the passage of fluids from the heart.

The experiments of Legallois show, that injuries extending to a considerable portion of the spinal cord kill by arresting the heart's action; and Wilson Philip has also proved that the same effect follows similar injuries of the brain. On the other hand, injuries of a circumscribed portion of the brain kill by destroying the sensation, the action of the heart being only arrested by the impediment to the passage of the blood through the lungs from the cessation of the respiration. We have here the important distinction between the nature of concussion and compression. No doubt, these are generally intermixed in injuries of the brain, but the symptoms of the one may preponderate over the other and demand its appropriate treatment. Many surgeons are decidedly opposed to the extraction of blood during the stage of collapse from concussion, when the pulse is small, rapid, and feeble. But we can easily suppose that during its weakened state, the blood which continues to flow along the veins, may embarrass the action of the right side of the heart, and interfere with the recovery of its contractility, and that opening the jugular vein, and allowing a few ounces of blood to escape from its lower orifice, would in some cases materially assist the action of the other remedies. There may be even a few rare cases in which the injury done to the central organs of the nervous system, may be just sufficient to arrest the contraction of the heart for a short time, and thus resemble the effects of the poison in the first experiment, where stimulants would be of no avail in restoring the heart's action, until the vein was first opened, and where, under proper management, the circulation might be restored.

Sir B. Brodie and others have shown, that poisons produce their fatal effects in one of two ways, either by suspending sensation, or by arresting the contractility of the heart—in other words, inducing death in a manner similar

to compression and concussion.[1] In these cases, where the action of the poison has only been sufficient to enfeeble transiently the contractility of the heart, opening the jugular, and allowing the blood to escape from the lower orifice, must materially assist the renewal of the heart's action. Of course, care must be taken that we do not, by the withdrawal of a certain quantity of blood, favour the absorption of the poison. It is, however, evident, that we can only expect a decided effect from the blood-letting in those cases, where the action of the poison has been transient, and where, either from the manner in which it has been applied, or from the interference of art, no repetition or renewal of its action can take place. The flow of blood from the lower orifice of the jugular vein, when opened, appears to depend upon two causes. 1*st*, Upon the contractions of the right side of the heart, more particularly when the blood in the veins is nearly stagnant, and the heart is congested. At each contraction, the heart attempts to force a certain quantity of blood along the vessels connected with it, and as there is no *vis a tergo* to prevent the action of the heart, moving the blood along the veins in its immediate neighbourhood, a certain quantity is forced out through the opening in the jugular. This I have often witnessed. During the congested and enfeebled state of the heart, the blood passes readily from the ventricle into the auricle, as Haller has remarked. 2*dly*, Upon the principle of derivation, as laid down by Haller, to whose observations upon this subject my attention was directed by Dr. Alison, after I had commenced these experiments.

Haller[2] ascertained by repeated experiments upon the

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[1] For an admirable exposition of the action of the causes of sudden death, see the second chapter of Dr. Alison's Outlines of Pathology.

[2] Sur le Mouvement du Sang. Opera Minora, tom. i., pp. 99-130. Lausannæ, 1762.

lower animals, that when a vein is opened the blood rushes towards the opening on the proximal side of the heart as well as on the distal, and from the veins communicating with the opened vein; that this in some cases extended to the neighbouring arteries after the blood had ceased to flow through them, and even after the blood appeared thickened in the veins; and that, as the blood continued to flow, the globules resumed their figure and their natural mobility. All this was found to take place independent of the heart, and after the aorta had been tied. These experiments were confirmed by Spallanzani.[1] Haller, as appears from different parts of his works,[2] was fully aware, that opening the jugular vein would empty the right side of the heart, and so assist the renewal of the circulation in asphyxia. He seems, however, to have attributed this entirely to the derivation of blood, as neither in the account of the experiments themselves, or in the deductions from these, does he make mention of the contractions of the heart. He was also aware, that congestion of the right side of the heart arrested its contractions sooner than if it had remained uncongested, but he makes no mention of the effect of unloading the heart in renewing its movements.[3] Indeed, the experiments of Haller and Spallanzani on the derivation of blood, made on the large veins in the neighbourhood of the heart, seem to have been liable to a source of fallacy in the renewal of the heart's action, of which they do not appear to have been aware. This could not, however, affect the accuracy of the other experiments made on other parts of

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[1] Experiments on the Circulation of the Blood. Translated by R. Hall, M.D., pp. 386-403. 1801.

[2] Sur le Mouvement du Sang, pp. 103, 104. 1756. Elementa Physiologiae, tom. ii. Lib. vi., sect. iv.

[3] Mémoires sur la Nature Sensible et Irritable des Parties du Corps Animal, tom i., p. 379. Exper. 545. Lausanne, 1756. Haller's observation applies to the auricle only.

the body upon this subject by those two distinguished individuals.

Since the engorging of the right side of the heart arrests its contractions sooner than they would otherwise stop, it will be necessary to bear this in mind in experimenting on the length of time during which the heart remains contractile after death, as the circumstance of the large veins being cut or kept entire, in laying open the thorax, may considerably modify the results.

[Results similar to those detailed in the above paper, have been since obtained by Dr. Cormack[1] and Dr. H. Lonsdale,[2] on dogs poisoned by creosote and prussic acid.[3] These gentlemen satisfied themselves that in certain cases the heart's action may be increased, or renewed, by opening the jugular vein at the lower part of the neck, and allowing the right side of the heart to become relieved of part of the blood distending it. The incomplete closure of the right auriculo-ventricular orifice by the tricuspid valve, especially when the right ventricle becomes distended, proved by the facility with which the right ventricle may be emptied through an opening in the jugular vein, is of some interest in a physiological point of view, and has been not inaptly termed by Mr. T. W. King, the *safety-valve* of the heart;[4] for, if this valvular apparatus was equally complete as that at the left auriculo-ventricular orifice, dangerous accumulations of blood might, under certain conditions of the body, take place in the vessels between the right and left sides of the heart. If the right ventricle, from a more perfect closure of its auriculo-ventricular orifice, conti-

[1] Treatise on Creosote, p. 84, 1836; and Prize Thesis on the Presence of Air in the Organs of Circulation, p. 38. 1837.

[2] Edinburgh Medical and Surgical Journal, No. 138.

[3] Dr. Lonsdale experimented only with the prussic acid.

[4] Guy's Hospital Reports, No. IV. April, 1837.

nued to propel the whole, or nearly the whole of its contents onwards along the pulmonary artery, under circumstances when the left side of the heart could not discharge equal quantities of blood into the aorta, an accumulation of this fluid in the pulmonary vessels must have ensued. The greater size of the cavities and of the auriculo-ventricular orifice of the right over the left side of the heart, is no doubt connected with the less complete valvular apparatus at the right auriculo-ventricular orifice, permitting a larger reflux of blood into the auricle on the right than on the left side, during the systole of the ventricles.

If this paper had been published after, instead of previous to that immediately preceding it, (No. II. on Asphyxia,) I would have expressed myself differently on a few points than I have done; but instead of commenting upon this, I shall content myself by stating that when two papers in this volume contain different views of any subject, the later published is presumed to be the more correct. In the memoir on asphyxia we state that the derangement of the functions of the *medulla oblongata*, and those of the *cerebrum* or the sensorial functions, are not necessarily coequal in extent, and never in importance in asphyxia, and this is well observed in some of those cases of death from disease or from narcotic poisons where the process of asphyxia occurs more slowly and gradually: and that it is evident from various facts that the arrestment of the muscular respiratory movements in such cases is not dependent upon the suspension of the sensorial functions, but upon those of the *medulla oblongata*. In the same manner, cases of compression of the brain prove fatal, not by suspending sensation, but by arresting the functions of the *medulla oblongata*.]

No. IV.

AN EXPERIMENTAL INVESTIGATION INTO THE FUNCTIONS OF THE EIGHTH PAIR OF NERVES, OR THE GLOSSO-PHARYNGEAL, PNEUMOGASTRIC, AND SPINAL ACCESSORY.[1]

(FROM THE EDINBURGH MEDICAL AND SURGICAL JOURNAL, JANUARY, 1838.)

THE Eighth Pair are undoubtedly the most interesting and important of all the nerves of the body, both in a practical and theoretical point of view. Their lesions are attended by the most serious derangement of the respiratory and digestive processes, and bear in a prominent manner upon some of the principal doctrines in physiology. The functions of these nerves have, therefore, commanded a more than ordinary degree of attention, and the industry and talent of numerous observers have been directed towards their elucidation. In entering upon an experimental investigation into the functions of the three divisions of this pair of nerves, I was fully aware of the numerous difficulties attending such an undertaking, and have endeavoured to approach it with all the circumspection and assiduity which its importance and inherent intricacies require. I,

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[1] A short epitome of this paper was read at the Meeting of the British Scientific Association, in September 1837.

nevertheless, feel very considerable diffidence in presenting the first results of this inquiry to the public, since the data which I have obtained will necessarily lead me to draw several important inferences regarding the functions of these nerves, at variance with those entertained by many of the most celebrated and experienced practical physiologists. And most persons feel disposed to receive with caution or even distrust the observations and opinions of one who can scarcely be said to have finished his novitiate in the difficult task of unravelling the operations of the complicated machinery of the higher organized bodies, and the more especially when a considerable part both of his premises and inferences are in opposition to those promulgated by physiologists, whose years and well-earned fame, acquired by their long and laborious exertions in the extension of this science, entitle their opinions to the highest credit. Being confident, however, from my previous pursuits, and from the great care with which I conducted, after their death, the dissections of all the animals operated upon, that I could not possibly be mistaken as to the particular nerves upon which the experiments had been made; profiting largely from the recorded errors and instructions of those who have preceded me in this field of inquiry; and taking the very necessary precaution of making a careful repetition of the experiments under varied circumstances, so as to avoid as much as possible any accidental sources of error, I have been encouraged to publish the results thus obtained—being satisfied that the facts will be found, upon actual examination, such, or at least nearly such, as I represent them. In entering upon this investigation I had no favourite theory to defend, stood committed to no preconceived notions, nor shackled by any slavish deference to authorities, but was ready and willing to give up any of my former opinions as soon as they appeared to be inconsistent with the phenomena which presented themselves; and I

sincerely hope that I am not chargeable with the opposite and equally dangerous fault of seeking and hankering after novelties.

In stating the experiments, I shall enter more fully into the details than many may think necessary, as it appears to me that it is an object of essential moment to mention all the circumstances under which any important experiment is performed in physiological investigations, where so many extraneous circumstances are apt to interfere with the results; and I am convinced that if this plan had been more fully followed, many a controversy might have been avoided, as well as much animal suffering spared, and the science would this day have presented fewer discordant statements, and less unfortunate collision of opinions. It may appear to some that I have repeated many of those experiments with unnecessary frequency, and a wanton sacrifice of animals. But I naturally felt diffidence and distrust in the accuracy of the results I obtained when opposed to those of more experienced observers, and it was only after repeated and careful examination of the phenomena, that I could feel myself justified in calling these in question. It is also sufficiently obvious, that nothing is more injurious to the progress of science than hasty and partial observations; and I was anxious to avoid, as far as I possibly could, adding to that mass of conflicting evidence which there is already so much reason to deplore. Besides, as every false observation requires additional experiments for its refutation, I felt that, with less extended opportunities of witnessing the phenomena under examination, I must incur a greater risk, not only of throwing obstacles in the way of the progress of truth, but also of occasioning a useless infliction of animal pain.

In detailing the following experiments, and the conclusions I feel inclined to draw from them, I shall follow the order in which the nerves, generally included under the

eighth pair, are enumerated, and therefore commence with the *Glosso-pharyngeal.*

PART FIRST.—GLOSSO-PHARYNGEAL NERVE.

The experiments on this nerve were all performed upon dogs, and were twenty-seven in number. Seventeen of these were for the purpose of ascertaining if it should be considered a nerve both of *sensation and motion;* and what are the effects of its section upon the *associated movement of deglutition*, and on the *sense of taste.* The other ten were performed upon animals immediately after they had been deprived of sensation, with the view of satisfying myself more thoroughly how far it is to be considered a *motor nerve.* In cutting down upon this nerve in the living animal, the following plan was adopted after a careful examination of the anatomy of the parts. An incision, varying in length in proportion to the size and configuration of the dog, was made nearly parallel to the back part of the base of the *inferior maxilla;* its posterior termination always extending a short distance over the anterior margin of the *sterno-mastoid muscle.* After cutting through the *skin* and *platysma myoides* or *cutaneous* muscle, the two large veins which go to form the *external jugular* were each secured in a double ligature, and cut across between the points tied. The *glands* at the angle *of the jaw* were removed or dissected aside, and the *carotid artery* exposed. The artery was generally first secured in a ligature below the *hypoglossal nerve*, and before it had given off any of its large branches; it was then exposed above the *hypoglossal*, included in a double ligature and cut across. This was found to be the most effectual method of preventing the troublesome hemorrhage, which otherwise is apt to occur from wounding the large arteries, arising from the carotid at this point. After this, by a little dissection, the nerve was exposed as it lies upon the lower margin of the *stylo-pharyngeous muscle*, near its

origin from the *styloid process*. One of the best guides in displaying the nerve is the *osseous expansion* connected with the *tympanic cavity* of the *temporal bone*, which is always readily felt after the *carotid* has been exposed. In dissecting the nerve back to the *foramen lacerum posterius*, great care must be taken to separate it from the *pharyngeal branch of the par vagum*, which lies sometimes in immediate contact with it, at other times one or two lines below, and is frequently united to it by a considerable communicating branch, so that it may readily be mistaken for a large pharyngeal branch of the glosso-pharyngeal. I am afraid, for reasons to be afterwards stated, that sufficient precautions have not been taken by other experimenters to separate these from each other. This is the more necessary, as I am confident that these two nerves differ very materially in function, and this must consequently have seriously affected the results. The superior laryngeal branch of the *par vagum* is placed a short distance below these two nerves, but was rarely seen except when exposed designedly. I have observed very considerable differences in the manner in which the glosso-pharyngeal gives off its branches to the pharynx and fauces: sometimes it begins to give off small filaments soon after it has emerged from the base of the cranium, and continues to give off a number of small branches in succession, until it has furnished all the filaments to the pharynx and fauces; more commonly the nerve forms a sudden enlargement upon the lower margin of the osseous expansion of the tympanic cavity; from this the principal branches to the pharynx and fauces are given off, one or two of which are much larger than the others. It is almost needless to add, that in all the animals kept alive, after section of these nerves for subsequent observations, a considerable portion of the trunk of each nerve was removed. The first point connected with the function of the nerve to which I directed my attention was the dis-

puted question, whether or not it is to be considered both a *motor* and *sensitive* nerve? In investigating this it has occurred to me, that the discordant results obtained in late experiments may be explained. It is with this object, as well as with the view of pointing out the difficulties I encountered at the outset, that I have here given the details of the first experiments, as they are put down in my notes taken at the time. These may serve as guides to those who may afterwards wish to turn their attention to this subject.

Is the *glosso-pharyngeal* a nerve both of motion and sensation?

1*st Exper.*—June 3, 1847.—The glosso-pharyngeal nerve was exposed on one side in a middle-sized terrier, and irritated by pincing it with the forceps, but no satisfactory evidence either of sensation or motion was observed, even when tied tightly in a ligature and cut across. When the same nerve was exposed on the opposite side, a powerful and sudden convulsive movement of the muscles of the throat and lower part of the face followed the first pinch of the forceps. After I had satisfied myself that this could be readily renewed by irritating the nerve, these muscular twitchings of the face appeared to me to resemble so closely those produced by pinching a branch of the *portio dura*, that though confident that the nerve exposed occupied the position of the glosso-pharyngeal, and that no branch of the *portio dura* lay along the lower margin of the stylo-pharyngeous muscle, yet I could not divest myself of the idea that some unusual distribution, or some other cause, had thrown a branch of the *portio dura* in our way. Another nerve, lying immediately below this, and of somewhat smaller size, was exposed, (which was afterwards ascertained to be the pharyngeal branch of the *par vagum*,) and experimented on under the supposition that it was the glosso-pharyngeal. When this nerve was irritated no distinct

contractions of the muscles of the throat were visible; and though the animal appeared to give some indications of suffering when the nerve was included firmly in a ligature and cut on its proximal side, yet these were far from being decisive.

2*d Exper.*—On exposing the glosso-pharyngeal nerve on one side in a young dog, the same convulsive movements of the throat and face were observed as in the preceding experiment. Being again considerably perplexed whether this could be the glosso-pharyngeal or not, since most of the numerous muscles thus thrown into convulsive action do certainly not receive any filaments from this nerve; while irritation of a branch of the *portio dura*, though sufficient to account for the convulsive movements of the face, could not, however, explain the extensive movements of the muscles of the throat, I tied a ligature loosely around it, to enable me to recognise it readily again, and then exposed the nerve lying immediately below, (pharyngeal branch of *par vagum*,) as in the preceding experiment. No convulsive movements were seen on irritating this last nerve, and the indications of suffering were not distinctly marked. The animal was then killed by blowing air into one of the exposed veins, and the anatomy of the nerves experimented on carefully examined, when we were satisfied, beyond the shadow of a doubt, that the nerve, the irritation of which was followed by such distinct convulsive movement in this experiment, was the glosso-pharyngeal. On exposing the pharyngeal branch of the *par vagum* and the glosso-pharyngeal on the opposite side in this animal, as soon as it had ceased to breathe, and when the constrictor muscles of the pharynx were much more fully displayed than during the operation upon the animal when alive, distinct contractions of these constrictor muscles were observed after each pinch of the pharyngeal branch of the *par vagum* with the forceps; while the same irritation, ap-

plied to the glosso-pharyngeal immediately afterwards, was followed by no visible muscular movement. Being now satisfied that in the preceding experiment we had in the second operation divided the pharyngeal branch of the *par vagum* instead of the glosso-pharyngeal, the animal was killed, and the dissection proved that we were right in our conjecture.

3d Exper.—The nerve was exposed on one side in a small cocker dog affected with the distemper, but no satisfactory evidences of sensation or muscular movements were observed. This might arise from the sickly condition of the animal, or, as we shall immediately show, from the nerve being only irritated anterior to, or on the distal side of that part of the trunk where the pharyngeal branches leave it.

4th Exper.—The glosso-pharyngeal nerve was first exposed on one side, and then on the other, in a middle-sized terrier. Very forcible convulsive movements of the muscles of the throat and lower part of the face, but more especially of the throat, were observed on pinching these nerves with the forceps. On cutting the nerve across, the same convulsive movements and indications of suffering were noticed on pinching the nerve on the proximal end, or that part of the nerve which retained its natural connexion with the *medulla oblongata.* These convulsive movements were exactly similar to those observed when the nerve was entire. There were no convulsive movements on irritating the lower end by the forceps.

5th Exper.—The nerve was exposed on one side in a middle-sized full-grown pointer. When irritated by pinching it firmly with the forceps, only very feeble convulsive movements of the throat were excited, and such as would readily have escaped notice, had we not been in search of them. Professor Sharpey, of the London University College, was present from the commencement of this experi-

ment, and as we were proceeding to expose the nerve of the opposite side, we were joined by Professor Alison. On irritating the opposite nerve by pinching it with the forceps, the convulsive movements of the muscles of the throat were distinctly marked, but still in a less decisive manner than in some of the preceding experiments. The nerve was then included firmly in a ligature. On irritating the nerve on the proximal side of the ligature, the same convulsive movements followed; while no effect was observed from the irritation of the nerve on the distal side, or that in connexion with the tongue and pharynx. The same difference in the results was also obtained from the irritation of the two ends of the nerve when cut across. We had in this dog an opportunity of contrasting the manner in which muscular movements are induced through the stimulation of a motor nerve, such as the hypoglossal, or the spinal accessory, with that motion which results from the excitation of a sensation, as appears to be the case when the glosso-pharyngeal is irritated. For while the stimulation of the hypoglossal and spinal accessory on the distal side of the ligature was followed by convulsive movements of the muscles to which they are distributed, and of these only, the stimulation of the glosso-pharyngeal on the same side of the ligature produced no visible effect. On the other hand, irritation of the hypoglossal and spinal accessory on the proximal side of the ligature excited no muscular movements whatever, while excitation of the glosso-pharyngeal on this side was followed by extensive movements in muscles, to the greater part of which it sends no filaments. When the glosso-pharyngeal was pricked with the forceps, the indications of suffering were distinctly but not strongly manifested, but the application of a tight ligature was evidently attended by intense pain—an effect which we were somewhat surprised to find also attended the application of

a tight ligature to the hypoglossal.[1] A fatal dose of prussic acid was given, and the muscles of the throat more fully exposed, when distinct convulsive movements of the constrictors of the pharynx were observed each time that the pharyngeal branch of the *par vagum* was pricked.

In dissecting the nerves after death, I was anxious to ascertain if any possible explanation could be given, why the convulsive movements attending the stimulation of the nerve should have been so feebly manifested on the side first operated on, and I ascertained that we had been experimenting on the nerve of this side beyond or on the distal side of the origin of nearly all its pharyngeal branches; while in the second operation we had been experimenting on the trunk of the nerve prior to, or on the proximal side of the origin of its pharyngeal branches. Dr. Sharpey examined these dissections, and expressed himself satisfied with the accuracy of the statements here given.

6*th Exper.*—The nerve was first exposed on the left side in a middle-sized terrier. On pinching the nerve the convulsive movements of the throat were rather feeble, though perfectly distinct. When proceeding to display the nerve on the right side we were joined by Professor Sharpey. We first pricked the lingual branch, by which I here mean the continuation of the trunk of the nerve proceeding to the tongue after giving off its pharyngeal branches, (under the belief that we were operating on the entire trunk,) and neither convulsive movements nor indications of suffering followed. On dissecting the trunk of the nerve backwards towards the *foramen lacerum*, and beyond the origin of the pharyngeal branches, distinct convulsive movements of the muscles of the face and throat, and decided indications of

[1] Mayer has described a small ganglion on the root of this nerve in the dog and ox. He has seen it once in the human subject.—See Edin. Med. and Surg. Journal, vol. xliii., p. 486.

suffering were observed. On pricking the nerve during the moaning of the animal, it was several times remarked that the voluntary actions of the muscles of the throat were interrupted by these convulsive starts.

This animal was killed a few days after, and it was ascertained that while the nerve had been satisfactorily divided on one side, one pretty large pharyngeal branch had been left on the other. Dr. Sharpey also examined this dissection, and was fully satisfied of its accuracy. The impression on my mind is, that this pharyngeal twig was found on the left side, (the side first operated on,) but as it is not expressly stated in my notes, I cannot positively affirm it.

7th Exper.—On exposing the glosso-pharyngeal nerve in a stout terrier bitch, we found that irritation of the lingual branch was attended by no visible muscular movements, and no decided indication of suffering; while on irritating some of the small pharyngeal filaments individually, given off by the trunk of the nerve soon after it leaves the *foramen lacerum*, convulsive movements of the muscles of the throat and face, and marked indications of suffering followed.

It will be unnecessary to detail the results of the other ten experiments performed to ascertain the effects of irritating this nerve, as far at least as the indications of muscular movements and common sensation are concerned. I may state, however, that the facts detailed in the above experiments were amply confirmed by those subsequently performed. It was found that the muscular movements excited by pinching this nerve varied in degree in different animals, but were always distinctly marked when the precaution was taken to irritate the nerve before it had given off its pharyngeal branches. It was also repeatedly observed, that when the nerve was cut across, irritation of the lower end, or that in connexion with the muscles, was

followed by no movement; while, on the other hand, irritation of the cranial end, even when the nerve was divided before it had given off a single twig, was followed by as powerful convulsive twitches as when the nerve was entire. We also remarked more than once, that while the pinching of the lingual branch caused no muscular movements, irritation of one of the small pharyngeal twigs was followed by very strong convulsive twitches of the throat and lower part of the face. The capability of this nerve to transmit those impressions which excite *sensation*, was amply proved by the severe and undoubted indications of suffering that in many cases followed the pinching of it with the forceps. In several cases, however, the indications of suffering were much less strongly marked than in others, but that this nerve possesses *common sensation*, in the usual acceptation of that term, I am fully convinced, not only from the results of these seventeen experiments expressly made upon these nerves, but from five others, where they were exposed in displaying the pharyngeal branches of the *par vagum*. That irritation of the trunk of the glosso-pharyngeal nerve is attended by convulsive muscular movements and indications of suffering, is in direct opposition to the statements of Panizza,[1] Dr. M. Hall, and the late Mr. Broughton,[2] and is in accordance with those of Dr. Alcock[3] of Dublin. I was at first fully convinced that these experimenters had been operating on different nerves, though it was difficult to conceive how such mistakes could arise in the hands of those so much accustomed to researches upon the nervous system. The details of Experiments 5th, 6th, and 7th, will,

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[1] Edinburgh Medical and Surgical Journal, vol. xlv., p. 86. 1836.

[2] Dr. M. Hall and Mr. Broughton in vol. iv., p. 679, of British Scientific Association; also Mr. Broughton in vol. xlv. of Edinburgh Medical and Surgical Journal, p. 429. 1836.

[3] Dublin Journal of Medical Science, vol. x., pp. 260-1. 1836.

I think, explain how such discrepancies might arise, without seriously impugning the anatomical knowledge or acuteness of observation of either party. For though I do not mean to affirm that pinching the lingual portion of the nerve is never followed by indications of suffering, (for from the irregularity in the origin of the pharyngeal twigs, and the difficulty of judging at the bottom of a deep wound, at what particular part these are all given off, it is generally impossible to decide when the lingual portion of the nerve may be said to begin,) yet I have no hesitation in saying, that if in these three experiments, and in others not detailed here, we had operated on that part of the trunk of the nerve which first presented itself, and not proceeded to dissect it back towards its place of exit from the cranium, we should have gone away with the impression that the irritation of this nerve was followed by no muscular movement, and little if any indications of suffering. Dr. Alcock[1] suggests that Panizza may have experimented upon the superior laryngeal branch of the *par vagum*, instead of the glosso-pharyngeal; but we can scarcely believe that an accurate anatomist like Panizza could commit such an error. Besides, in repeated experiments upon the laryngeal nerves (as we shall afterwards more particularly mention) we found, in all the animals operated on, except two dogs which appeared considerably exhausted by great previous suffering, ample grounds for dissenting from the statements of Dr. Alcock, that this nerve is devoid both of sensibility and of muscular influence. With the exceptions mentioned, very severe indications of suffering, and in a few cases also distinct muscular twitching of the neck and face attended the pinching and cutting of this nerve. These muscular twitchings, however, were certainly not so fre-

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[1] Op. cit., p. 266.

quently observed nor so well marked as those we witnessed from pinching the glosso-pharyngeal.

Can these muscular movements attending the irritation of the trunk of the glosso-pharyngeal nerve arise partly or wholly from the development of one of those impressions which, when conveyed by the sensitive filaments to the central organs of the nervous system cause some influence to be transmitted along certain other motor nerves, by which particular muscles are called into action—in the same manner as irritation of the trunk of the optic nerve causes contraction of the iris, and compression of the trunk of the *par vagum* in the neck not unfrequently produces violent movements of the respiratory muscles? Or can it be that these muscular movements depend partly or wholly upon the irritation of the nerve acting directly through its motor filaments upon the muscular bundles to which they are distributed—in the same manner as pinching of the hypoglossal causes convulsive movements in the muscles of the tongue? Dr. Alcock is doubtful on this point. Though "disposed to regard the result in question as the effect of a sentient impression excited through the nerve, and referred to the interior of the pharynx," from the fact that this movement extends to muscles not supplied by this nerve, and forms an associated action, he yet admits "that the circumstance may be as well explained by an exalted degree of muscular excitement, or by a higher one than that necessary to produce the simple starting."[1] In the experiments which I have detailed above, we have, however, sufficient evidence to decide that these muscular movements can only be explained on the supposition that they depend upon some influence transmitted along other nerves distributed to these muscles, and through the intervention

[1] Oper. cit., p. 265.

of the central organs of the nervous system. We there find, and the facts were still farther confirmed by what we observed in other experiments, that irritation of a single pharyngeal twig, or, what is still more conclusive, of the upper or cranial end of the cut nerve before it had given off any of its branches, was attended by as powerful convulsive movements, as when the trunk of the nerve and all its branches were entire and uninjured.

Is this nerve, then, entirely a nerve of sensation, or is it partly motor and partly sensitive?

Mr. Mayo[1] and Professor Müller[2] maintain that it is both a nerve of sensation and motion—the former on physiological, the latter on anatomical grounds. "When the *glosso-pharyngeal* nerve," says Mr. Mayo, "is pinched in an ass recently killed, a distinct convulsive action ensues, apparently including and limited to the *stylo-pharyngeous* muscle, and the muscular fibres about the upper part of the *pharynx*." I have already stated in the above experiments that no muscular movements were observed on irritating the lower end of this nerve when cut across. I attach, however, little weight to this observation, since it was but rarely possible to see distinctly the constrictors of the pharynx, from the imperfect manner in which these muscles were exposed in operating on the living animal. As it is agreed that when a motor nerve is irritated immediately after death, and while the muscular contractility is still vigorous, the muscles in which it is distributed are thrown into contraction, I proceeded, therefore, as in the above experiment by Mr. Mayo, to satisfy myself on this point. These experiments were made upon ten dogs, the nerves being exposed immediately after death, and the irritation of the pharyn-

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[1] Anatomical and Physiological Commentaries, No. ii., pp. 11, 12. 1823.
[2] Müller's Archiv für Anatomie, Physiologie, &c. 1837. S. 275-7.

geal branch of the *par vagum* was always taken as a test that the muscular contractility was still in sufficient vigour to be readily excited through the motor nerves, and also as a standard of comparison. I cannot say that the results of these experiments were uniform, but I am convinced that in none of them was there any satisfactory evidence to lead us to believe that the glosso-pharyngeal is a nerve of motion; and I am perfectly satisfied that in all of them which were quite accurately performed no muscular movements were seen on irritating this nerve. As the experiment is one which requires great care in its performance, and is liable to a marked source of fallacy from the intimate connexion of the nerve with another, viz. the pharyngeal branch of the *par vagum*, undoubtedly a nerve of motion, I shall relate some of the experiments which illustrate this. Before doing this I may again more particularly refer to the relative position of these two nerves to each other, since this is not mentioned, as far as I am aware, by any previous experimenter. The trunk of the *pharyngeal branch of the par vagum* lies, as we have already stated, quite close to that of the *glosso-pharyngeal*. It divides into ascending and descending branches, the ascending branch (or branches) passes to the superior constrictor and muscles about the isthmus of the fauces, runs generally immediately behind the trunk of the glosso-pharyngeal for a short distance, and may remain merely in close apposition, or form a free anastomosis with it. Very frequently the trunks of the two nerves anastomose by a short branch a little anterior to the *foramen lacerum*, in which case it generally requires a careful dissection to show that the large descending twig of the pharyngeal branch of the *par vagum* is not a part of the glosso-pharyngeal nerve. In one case I dissected lately, a communicating branch connected the trunk of the two nerves before they had fairly cleared the *foramen lacerum*. It is absolutely necessary to have these facts impressed upon

the mind before any satisfactory experiments can be made upon the glosso-pharyngeal, with the view of ascertaining whether it is a motor nerve or not.

8*th Exper.*—The *glosso-pharyngeal nerve*, *pharyngeal branch of the par vagum*, and *constrictor muscles* of the *pharynx*, were exposed in a middle-sized mongrel, immediately after it had been deprived of sensation by a dose of prussic acid. On pinching the trunk of the glosso-pharyngeal nerve no muscular movements were observed, while on pinching the *pharyngeal branch* of the *par vagum*, immediate and powerful convulsive movements of the *constrictor muscles* of the *pharynx* followed. This was repeated several times on both sides with the same results.

The same phenomena were witnessed in two other dogs, with this additional observation in one of them in which the anterior part of the pharynx was opened, that the muscles about the *isthmus* of the *fauces* as well as the *constrictors* of the *pharynx* were thrown into action on pinching the *pharyngeal branch* of the *par vagum.*

9*th Exper.*—The *glosso-pharyngeal*, *pharyngeal branch* of the *par vagum*, and *constrictors* of the *pharynx*, were exposed in a young terrier immediately after a fatal dose of prussic acid had been given. On pricking the glosso-pharyngeal no effect followed, while convulsive movements of the constrictors of the pharynx were very apparent on pricking the pharyngeal branch of the *par vagum.* The wires of a pretty powerful galvanic trough were then applied, and it was observed that when proper precautions were taken to insulate the glosso-pharyngeal, so as to avoid the passage of a current through the muscles, no movement was seen, while very powerful convulsive movements of the *pharynx* attended each application of the wires to the *pharyngeal branch* of the *par vagum.* The nerves of the opposite side were then exposed. At the first application of the galvanic wires to the glosso-pharyngeal no movement followed. On

repeating the application, slight irregular movements were observed in the middle *constrictor*, where many of the *pharyngeal* branches of the glosso-pharyngeal enter it. The same extensive and vigorous movements, as were observed on the opposite side, followed each application of the wires to the *pharyngeal branch* of the *par vagum*. We were afterwards satisfied that the slight convulsive motions seen on irritating one of the glosso-pharyngeal nerves in this experiment, could be accounted for by the transmission of a slight current through the muscle, for in this case the trunk of the nerve was not cut across, but merely raised on an aneurism needle. Now Mr. K. T. Kemp (whose practical acquaintance with every thing relating to galvanism is well known) to whom I referred the question, stated to me that it was perfectly possible, nay probable, that a part of the galvanism generated by a battery of twelve double plates, each five inches square, such as was used in this experiment, would, instead of passing across between the two wires, take the more circuitous course along the circle, which was formed through the muscle by the conducting nerve and cellular tissue. In fact, we had demonstrative evidence that such might be the case in this experiment, for my friend, Dr. J. Duncan, pointed out at the time, that if the aneurism needle was allowed to touch the sterno-mastoid muscle, strong convulsive movements of this muscle were excited, though the needle was placed under the nerve a little posterior to the part where the wires were applied.

10*th Exper.*—The *glosso-pharyngeal nerve*, *pharyngeal branch* of the *par vagum* and *constrictor muscles* of the *pharynx*, were exposed in a young dog immediately after it had been deprived of sensation by a fatal dose of prussic acid. Distinct convulsive movements of the muscles of the pharynx were seen when the pharyngeal branch of the *par vagum* was pricked by the forceps; no visible movement when the glosso-pharyngeal was similarly treated. The

nerves were then cut across, and galvanism applied with the same results. The same observations were made upon the nerves of the opposite side. Dr. Alison was present at this experiment.

11*th Exper.*—The *nerves* and *constrictor muscles* of the *pharynx* were exposed on one side in a middle-sized dog, immediately after it had been killed. Pinching the *glosso-pharyngeal* was followed by convulsive movements of the upper part of the constrictors of the pharynx, and stylo-pharyngeous muscle, but on separating a large twig of the pharyngeal branch of the *par vagum* which lay below it, and which was also embraced by the forceps, no effect followed from irritating the *glosso-pharyngeal* alone. Pinching the pharyngeal branch of the *par vagum* was followed by vigorous movements of all the *constrictors* of the *pharynx* and *stylo-pharyngeous* muscle. The same observations were repeated on the opposite side.

In two other experiments convulsive movements in the *stylo-pharyngeous* muscle and *upper part* of *pharynx*, such as was observed by Mr. Mayo in the ass, followed the pinching of the *glosso-pharyngeal;* but in these the nerves had been cut through hurriedly, and when covered with blood, so that it was afterwards impossible to say whether the large ascending twig of the pharyngeal branch of the *par vagum* was included along with the glosso-pharyngeal or not. The movements observed were such as in other cases attended the irritation of that twig of the pharyngeal branch of the *par vagum.* Recalling to my recollection these facts, and the result of Experiment 11th, I determined to repeat my observation in a more accurate manner.

12*th Exper.*—The nerves and pharyngeal muscles were exposed in a large Newfoundland dog, after receiving a poisonous dose of prussic acid. The glosso-pharyngeal nerve was cut across and galvanism applied, but this was so close to the communicating twig of the pharyngeal branch

of the *par vagum*, that no accurate observations could be made. The nerve was then rapidly but carefully displayed on the opposite side without disturbing it, and it was observed that when it was pricked with the forceps posterior to, or on the proximal side of the point where it was joined by the communicating branch, so often mentioned, no movement resulted: on the other hand, the application of the forceps at a certain distance anterior to or on the distal side of this junction, was followed by convulsive movements of the upper part of the pharynx. On irritating the pharyngeal branch of *par vagum*, rapid and vigorous movements of all the pharyngeal muscles and upper part of the œsophagus followed.

13*th Exper.*—The difference between the effects of irritating the *glosso-pharyngeal*, anterior and posterior to the junction of the communicating branch, was also observed in this experiment performed upon a young dog. The same extensive movements of the *constrictors* of the *pharynx* and upper part of the *œsophagus* were again witnessed on pricking the *pharyngeal branch* of the *par vagum*. On opening the anterior part of the *pharynx*, these movements were also seen to extend to the muscles of the isthmus of the fauces and soft palate, as indicated by the motions of those parts. Irritation of the *glosso-pharyngeal* was again repeated when the *pharynx* had been opened, but still no muscular movement could be detected.

The preceding experiments appear to me sufficient to prove that the glosso-pharyngeal cannot be considered a motor nerve. I am perfectly aware that negative are infinitely less conclusive than positive experiments, and that one well ascertained positive will outweigh a whole host of negative experiments, so that if I had been satisfied even in one of the ten experiments that muscular movements followed the irritation of the glosso-pharyngeal, when fairly insulated from the pharyngeal branch of the *par vagum*;

or if these experiments did not furnish a sufficiently plausible explanation of the cause of the discrepancy between these results and those obtained by Mr. Mayo upon the ass, I must have been compelled to admit that the glosso-pharyngeal was also a motor nerve, though to a limited extent. The conclusions I have formed regarding the difference in function between the glosso-pharyngeal and pharyngeal branch of the *par vagum* are also greatly supported by their ultimate distribution upon the *pharynx* and *fauces*, as I shall afterwards point out when we come to the consideration of the functions of the pharyngeal branches of the *par vagum*.

With regard to the argument in favour of the motor properties of this nerve, drawn by Müller from its anatomy, it appears to me that this analogical mode of investigation, valuable though it be, must be permitted to yield to the more positive observations obtained from experiments. Of the existence of the *ganglion jugulare N. glosso-pharyngi*, which was first pointed out by Ehrenritter,[1] and more lately described by Müller;[2] and of its apparent limitation to the posterior filaments of the nerve, I am fully convinced from actual examination. And though it must be granted that this nerve here resembles very closely the double roots of the spinal nerves, yet we must be wary in drawing analogies between the glosso-pharyngeal and the spinal nerves, for we have another ganglion situated immediately below this, viz. the *ganglion petrosum* of Andersch, involving the whole trunk of the nerve; and to this assuredly we have no analogical structure in the spinal nerves, if we admit that the superior ganglion resembles those upon their posterior roots. Müller no doubt supposes that this inferior ganglion

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[1] Salzburger Medicinisch-chirurg. Zeitung Band iv. S. 319. 1790.
[2] Archiv für Anat. und Physiol. 1834. S. 11.

differs from those placed upon the posterior roots of the spinal nerves, and that it belongs to the sympathetic system. But as nothing like conclusive proof is advanced in support of this opinion, we may, in the meantime, reasonably suspend our belief as to the probable influence which this lower ganglion may exert upon the functions of the nerve.[1]

Since, then, we are led to believe that the glosso-pharyngeal is entirely a nerve of sensation, and that the muscular movements which result from its irritation depend not upon any influence extending downwards along the branches of the nerve to the muscles moved, but to a reflex action exerted through the medium of the central organs of the nervous system, we have next to inquire whether or not these muscular contractions resemble any of the instinctive associated muscular movements concerned in the Function of Deglutition. It appeared to Dr. Alcock that they strictly resembled the associated movements "ordinarily excited by a disagreeable sensation experienced in the fauces and pharynx."[2] There is little doubt that the muscles thus thrown into action are those concerned in regulating the course of the ingesta along the pharynx; but I have not, after very frequent opportunities of carefully watching this movement, been able to satisfy myself that it resembled very closely any of the associated movements usually engaged in this function. If asked, however, to which of them it most nearly approximated, I would say, to a rapid act of deglutition, with this difference, that in the action of deglutition there is much more extensive movement of the larynx and lower part of the pharynx indicated by

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[1] Is it possible that the filaments which do not pass through the *ganglion jugulare* can be connected with the specific sensations of the fauces and pharynx?

[2] Op. cit., p. 264.

the ascent and subsequent descent of the hyoid bone and thyroid cartilage. I endeavoured in several of the experiments, by gently pricking, pulling, and pinching the nerve, to produce the more slow and usual effects of deglutition, or of those "excited by disagreeable sensations in the fauces and pharynx;" but without success. From what I have already said it appears that the excitation of these extensive muscular movements is connected with the *pharyngeal portion* of the nerve; and under the term pharyngeal, I here include a great part of the filaments going to the fauces. I am satisfied that this phenomenon is essentially different from one of those rapid involuntary muscular movements not unfrequently produced by the sudden excitation of pain. I have watched the effects of cutting and otherwise irritating some of the superficial nerves of the cervical plexus which are exposed in displaying this nerve, and I am convinced that they cannot be classed under the same head. The muscular movements of the throat and face observed on irritating the glosso-pharyngeal were sometimes as well marked when the animal was otherwise quiescent, as when also attended by the general struggles of the animal. We believe, then, that these pharyngeal filaments possess specific endowments connected with the peculiar sensations of the mucous membrane, upon which they are distributed, though we cannot pretend to speak positively in what these consist. The apparent difference in the endowments of the *pharyngeal* and *laryngeal* branches might readily suggest some speculations upon the differences in the sensations of those portions of the mucous surface upon which they are ramified, but from these we at present abstain.

The next subject of inquiry which naturally presents itself is, what effect has section of the glosso-pharyngeal nerve upon the *functions of deglutition?* I certainly supposed, after witnessing these extensive movements of the muscles of the throat and lower parts of the face, excited by

irritation of the glosso-pharyngeal, that upon this nerve must depend those excitations of the fauces and pharynx which give rise to the associated movements of deglutition. I therefore fully anticipated that section of this nerve would seriously interfere with the proper performance of these functions; and it was only after I could no longer resist the facts ascertained by my subsequent experiments, that I reluctantly abandoned the idea. The chief embarrassment experienced in arriving at satisfactory results on this point, was the great difficulty of dividing the nerve before it had given off any of its branches. To display the trunk of the nerve became to us a comparatively easy process; but to cut it close to the *foramen lacerum*, and before it had given off any of its twigs, remained to the last a matter of great perplexity. It was only after repeated failures that we fairly succeeded in effecting this on both sides, and I was never fully satisfied that the nerves were satisfactorily divided, until I had carefully dissected the parts after the death of the animal. After we had succeeded in exposing the trunk of the nerve, and traced it back to the neighbourhood of the *foramen lacerum*, it was always necessary to proceed with great caution, as it here lies in close apposition with the trunks of the *par vagum*, spinal accessory, hypoglossal, and the sympathetic; and when to the difficulty of separating parts thus placed so closely to each other, and lying at the bottom of a deep wound, we add the obscurity so frequently arising from the blood poured out from the division of the numerous small vessels surrounding these nerves, and the frequent and violent struggles of some of the animals whenever the glosso-pharyngeal was seized by the forceps, some notion may be formed of the difficulty of succeeding in this attempt. I ascertained that several of the animals, when all the branches of the nerve had been fairly divided except one or two of the small twigs going to the pharynx, could nevertheless swal-

low perfectly in from ten days to a fortnight; in other words, after the pain and swelling arising from the incisions had considerably abated. As, however, I had seen equally vigorous muscular movements excited by pinching one of the *pharyngeal twigs*, as from the trunk of the nerve itself, I considered these unsatisfactory. In three dogs, which lived long enough after the perfect section of the trunk on both sides to enable me to make decisive observations, the power of swallowing was perfectly retained. The most satisfactory of these I shall here shortly detail.

14*th Exper.*—The glosso-pharyngeals were divided in a middle-sized young terrier. This animal recovered rapidly from the effects of the operation, and a few days after it swallowed small morsels readily. Ten days after, it swallowed large masses with great facility, notwithstanding the wounds in the neck were still open. Fourteen days after the operation, it was repeatedly tried with morsels of various sizes, and there could be no doubt that it swallowed large masses quite readily and as perfectly as ever it did. These observations were again repeated to the perfect satisfaction of all those present. It was then killed, and a careful dissection made of the nerves experimented on. The upper cut ends of the glosso-pharyngeal nerves were found lying within the *foramen lacerum*, and not a single filament arose from either nerve above the point where they had been divided. More than an inch of each nerve had been removed. This last experiment, being a positive one, is alone sufficient to decide, independent of the other two which I might also detail, that the glosso-pharyngeal is not the sole nerve upon which the sensations of the fauces and pharynx necessary to the act of deglutition depend, though I believe there can be little doubt, both from its extensive distribution upon the mucous surfaces of these parts, and from the evidence afforded by the experiments upon the effects of pinching its pharyngeal branches, that

it must be concerned in exciting those sensations. Other nerves of sensation are distributed upon these surfaces, viz. the descending palatines of the second branch and a few filaments from the lingual portion of the third branch of the fifth, upon the mucous membrane of the *soft palate* and *isthmus* of the *fauces;* and branches of the *laryngeal* nerves, but principally of the *superior laryngeal* upon the mucous membrane of the *pharynx*. What would be the result of cutting all these nerves I have not yet attempted to ascertain, but having first satisfied myself that section of the *superior laryngeals* does not interfere with the act of swallowing, I performed the following experiment :—

15*th Exper.*—Both the *glosso-pharyngeals* and *superior laryngeal* branches of the *par vagum* were cut in a stout terrier dog. Shortly after the operation he swallowed several pretty large morsels of animal food quite readily. Next day he also swallowed two or three pretty large morsels, in such a manner as to satisfy me, that if the swelling and inflammation were subdued, his powers of deglutition would not be impaired. On the third day he was seized with pneumonia, and died before any other accurate observations could be made. On dissection, these two nerves were found to be satisfactorily divided on both sides. This experiment was repeated upon another dog, but this animal died two days after, without even attempting to swallow. I, however, feel so confident, from what I saw in the first dog, that the section of both these nerves would not affect the associated movements of deglutition, that I have not thought it necessary to repeat the experiment. The inference to which these experiments naturally leads us, is, that when the palatine branches of the fifth pair are uninjured, these are sufficient of themselves to furnish the sensations upon which the associated muscular movements of deglutition depend.

The results thus obtained regarding the effects of section

of the glosso-pharyngeal upon the function of deglutition differ considerably from those observed by Dr. Alcock; for, according to that gentleman, in those instances in which the nerve was perfectly divided "the deglutition was so much interfered with as in some cases to be impossible,"—"it (the animal) often experienced so much difficulty as to become much exhausted, or to seem even in danger of suffocation before it succeeded."[1] The experiments which I have detailed above are sufficient to show that these effects are not the necessary consequence of the perfect section of these nerves. And as the results obtained by Dr. Alcock were only observed by me when the *pharyngeal* branch of the *par vagum* was divided, never when it was left uninjured; and as I shall afterwards show, that the section of this nerve alone is sufficient to produce these effects, I am irresistibly led to conclude that this gentleman must have divided that nerve along with the glosso-pharyngeal. Indeed, this might be inferred from his own words, for in criticizing the experiments of Panizza, and attempting to point out the sources of the errors into which he supposes him to have fallen, Dr. Alcock describes the glosso-pharyngeal in such a manner as to leave no doubt in my mind, after frequent and careful dissection of the parts, that his *pharyngeal* branch of the glosso-pharyngeal is really the *pharyngeal* branch of the *par vagum*. As I have already pointed out, the relation of these two nerves to each other is so close, that it is only by frequently tracing them to their origin that we are able to distinguish them readily in all cases.

It is unnecessary to state how much these experiments are at variance with the opinion of Sir C. Bell, that the function of this nerve is to associate the movements of the tongue and pharynx with the muscles of respiration in the

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[1] Oper. cit., p. 261.

instinctive movements of deglutition. It is obvious from the experiments already and to be subsequently mentioned, that these associated movements of the *tongue* must depend upon the *hypoglossal*, and those of the *pharynx* upon the *pharyngeal branch* of the *par vagum*. Mr. Shaw states[1] "that the power of this nerve over the pharynx has been shown by several experiments, the results of which are very curious, and corroborative of the views (viz. of combining the movements of the tongue and pharynx) deduced from comparative anatomy." What these experiments were we are not informed; but it must be obvious from those related above, that the derangements of these movements were not necessarily connected with lesion of this nerve. Besides, the anatomical fact that the lingual portion of this nerve is distributed entirely, or almost entirely, to the mucous surface of the tongue, is sufficient to entitle us to call in question its alleged motor powers over that organ, if it be supposed by the supporters of this opinion to act as a motor nerve.

We have lastly to inquire in what manner the section of the glosso-pharyngeal nerve affects the sense of taste. My observations on this head are in perfect accordance with those of Dr. Alcock. Dr. Alison had an opportunity of witnessing the persistence of the sense of taste in one of the dogs, after a portion of the trunk of the nerve on both sides had been removed, and Dr. Sharpey was perfectly satisfied that the animal, the subject of the 6th Experiment, was sufficiently sensible of disagreeable impressions upon this sense. And though in the case witnessed by Dr. Alison a few pharyngeal filaments, and in that witnessed by Dr. Sharpey one pharyngeal twig on one side, were found to have been left uncut, yet it was obvious that the rejected morsel sprinkled with coloquintida was fully recognised before it

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[1] London Medical and Physical Journal, vol. xlix. p. 453. 1823.

passed beyond the anterior part of the mouth. I need not add that the lingual portion of the nerve was fully divided in both of these cases. The remark, however, was repeatedly made, (and it is of importance, as explaining the error of Panizza on this point,) that if animal food was offered, and the dog very hungry, he would eat the morsel containing the coloquintida rather than lose it; though he refused it if he saw any prospect of procuring another free from the bitter. The subject of the 1st Experiment, in which, as was stated, the glosso-pharyngeal was cut on one side only, even eat readily several pieces of bread dipt in a strong infusion of gentian root. Lest any doubt may arise that the presence of a few pharyngeal branches could have influenced the sense of taste, I may adduce the subject of the 14th Experiment, to prove that when the nerve is divided before it has given off a single filament, still the animal retains a sufficiently acute perception of disagreeable savours. I have fed that dog with morsels of animal food from my hand; and after he had taken several morsels in this way, which he readily swallowed, I then presented a morsel similar in size to the others, and with the coloquintida concealed in a way that he could not see it, but no sooner was it taken into the mouth than it was rejected with evident symptoms of disgust. This was repeated more than once. I find that Müller, in the second Number of his Archives for this year, (1837,) states that some experiments were performed in Berlin, in the summer of 1836, by Dr. Kornfeld, in which persistence of the sense of taste was ascertained after section of this nerve. It is very probable, nevertheless, that this nerve, though not the special nerve of the sense of taste, as supposed by Panizza, Dr. M. Hall, and Mr. Broughton,[1] may yet participate in this

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[1] Sixth Report of British S. Association, p. 125 of Transactions of the Sections.

function, along with the lingual portion of the third branch, and the palatine twigs of the second branch of the fifth pair. This view is not only supported by the anatomical fact that the mucous membrane and papillæ of the tongue, for about an inch in front of the *foramen cœcum*, are almost entirely supplied by this nerve, but also by the experiments of Dr. Alcock.[1] I endeavoured to ascertain the state of the sensibility, and of the sense of taste in that portion of the tongue where this nerve is ramified, after the trunk had been divided on both sides; but from the restlessness and struggles of the animals, I was unable to arrive at any satisfactory results.

From a review of all the experiments I have performed upon the *glosso-pharyngeal nerve*, I am inclined to draw the following conclusions:—

1. That this is a nerve of *common sensation*, as indicated by the unequivocal expression of pain by the animal, when the nerve is pricked, pinched, or cut.

2. That mechanical or chemical irritation of this nerve before it has given off its pharyngeal branches, or of any of these branches individually, is followed by extensive *muscular movements* of the throat and lower part of the face.

3. That the muscular movements thus excited, depend, not upon any influence extending downwards along the branches of the nerve to the muscles moved, but upon a *reflex action*, transmitted through the central organs of the nervous system.

4. That these *pharyngeal branches of the glosso-pharyngeal* possess endowments connected with the *peculiar sensations* of the mucous membranes upon which they are distributed, though we cannot pretend to say positively in what these consist.

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[1] Oper. cit.

5. That this cannot be the sole nerve upon which all these sensations depend, since the perfect division of the trunk of the nerve on both sides does not interfere with the perfect performance of the *function of deglutition.*

6. That mechanical or chemical irritation of the nerve, immediately after the animal has been killed, is not followed by *any muscular movements,* when sufficient care has been taken to insulate it from the *pharyngeal branch of the par vagum.* And we here again observe an important difference between the movements excited by irritation of the *glosso-pharyngeal* and those of a motor nerve. For while the movements produced by the irritation of the *glosso-pharyngeal* are arrested as soon as the functions of the central organs of the nervous system have ceased, those from irritation of a motor nerve, such as the *pharyngeal branch of the par vagum,* continue for some time after this, and when all connexion between it and the *medulla oblongata* has been cut off.

7. That after perfect section of the nerve on both sides, the *sense of taste* is sufficiently acute to enable the animal readily to recognise bitter substances.

8. That it probably may participate with other nerves in the performance of the function of taste, but it certainly is not the special nerve of that sense.

Lastly, The *sensation of thirst,* which is referred to the *fauces* and *pharynx,* does not appear to depend entirely upon the presence of this nerve. The animals in which it was divided lapped water of their own accord. I observed one of them, where the nerves were found satisfactorily divided, rise, though very feeble, walk up to a dish containing water, lap some of it, and return again to the straw upon which he was previously lying.

To Dr. J. Duncan and Mr. J. Spence, I am most deeply indebted for their valuable assistance in the performance of these experiments upon the glosso-pharyngeal nerve and

the greater part of those to follow. It is obvious, that without the aid of active and intelligent assistants, it would have been perfectly impossible to have proceeded with such an investigation. These two gentlemen witnessed the facts stated in the preceding experiments, and I have their sanction for their accuracy.

PART SECOND.—*Pneumogastric Nerve.*

I shall first consider the immediate effects of the mechanical and chemical irritation of that part of the trunk of this nerve which lies in the neck, and then proceed to the separate investigation of the functions of its *pharyngeal*, *laryngeal*, *œsophageal*, *cardiac*, *gastric*, and *pulmonary* branches.

I have exposed the trunk of the *par vagum* in the neck in at least thirty animals, and in almost all of these, the pinching, cutting, and even the stretching of the nerve were attended by *indications of severe suffering.* It was frequently difficult to separate the nerve from the artery, on account of the violent struggles of the animal, though some of them had been pretty quiet during the previous part of the operation. It has appeared to me, however, that a considerable degree of stretching and even compression may in many cases be exercised, without exciting any apparent suffering, when these are gradually applied. And this may perhaps account for the statement of Dr. M. Hall and Mr. Broughton,[1] that "in pinching the *par vagum* neither of the phenomena above-mentioned (viz. sensibility and muscular movements) occurs." Very few of the numerous experimenters upon the *par vagum* say anything about the sensibility of this nerve, apparently because their investigations were almost always conducted with a view to ascertain the effects of its section upon the circulatory, respiratory, and

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[1] Reports of British S. Association, vol. iv., p. 677.

digestive functions. I find, however, that Haller, in some experiments expressly undertaken for the purpose of ascertaining the degree of sensibility possessed by the nervous trunks, observed unequivocal signs of suffering on injuring the pneumogastric nerves. In relating an experiment upon a rabbit, he says—" Utcunque nervorum octavi paris, resecuimus, miseris cum doloribus, et contorsionibus animalculi."[1] Again, in describing the effects of the application of a ligature upon these nerves in another rabbit, it is mentioned—" ejulavit inter vinciendum miserum animal."[2] Brunn, in detailing several experiments upon the effects of including the *par vagum* in a ligature, describes the animals as giving undoubted evidence of feeling pain.[3] Dupuy states, in giving the details of one of his experiments, that " l'animal témoigna beaucoup de douleur pendant la division du nerf."[4] Dumas also, in relating an experiment upon this nerve, informs us, that when " on passe ensuite une ligature autour du nerf pneumogastrique ; il témoigna par ses mouvements et par ses cris la vivacité de sa douleur."[5] It is stated by Dr. M. Hall and Mr. Broughton,[6] that when the compression of this nerve is continued " for a few moments, an act of *respiration and of deglutition follows, with a tendency to struggle and cough.*" I have frequently repeated this experiment; and though in some of the animals powerful respiratory movements were produced by compressing the nerve, followed soon after by struggles, yet I have never observed either any tendency to cough or any act of deglutition, which I could fairly refer to this

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[1] Opera Minora, tom. i., p. 360., Exp. 136. Laus. 1762.

[2] Oper. cit., tom. i., p. 359., Exp. 132.

[3] De Ligaturis Nervorum, Ludwig, tom. ii., Scrip. Nov. Min. Sel., pp. 285-7.

[4] Journal de Médecine, Chirurg., &c., Dec. 1816, p. 359, Exp. iv., tom. 37.

[5] Journal Général de Médecine, tom. xxxiii., p. 355.

[6] Oper. cit., 677.

cause. The most satisfactory of these experiments I may here shortly relate.

16*th Exper.*—The pneumogastric nerves were exposed in a middle-sized mongrel dog. On laying hold of the nerve with the forceps, rather gently at first, but soon increasing the pressure so as to squeeze it pretty firmly, no effect was observed for a few seconds, but the breathing then became somewhat heaving, with a noise resembling snoring. This was repeated twice on each nerve, and always with the very same effects. The struggles attending the compression of the nerve in this animal were very slight.

Bichat contends that the increased respiratory movements accompanying the irritation of the *par vagum* depend solely upon the sudden excitation of pain.[1] Unquestionably the sudden infliction of pain hurries the respiration; but I believe that there can be no doubt that Bichat was in error, when, to this circumstance alone he attributed all the increased respiratory movements observed on irritating the pneumogastric nerves. In case it may be argued that these increased respiratory efforts arise from the partial arrestment of the movements of the glottis consequent upon compression of one of these nerves, I may state that I have seen them in two cases when the animal was breathing through a large opening in the trachea. That these increased respiratory movements are not dependent upon any direct effect which irritation of this nerve has upon the lungs or thoracic muscles, is proved by the fact, that when the nerve is cut across, the irritation of the portion in connexion with the *medulla oblongata* alone excites these movements.

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[1] Sur la Vie et la Mort, p. 296. 3d edition. Paris, 1805. Bichat in other passages admits the disturbance of the respiratory movements after lesion of these nerves, independent of the pain and terror of the operation. Vide Opus cit., p. 297.

Before proceeding to the consideration of the function of the pharyngeal branches of the *par vagum*, I may here briefly advert to the effects of section of the *par vagum* upon the conjunctival membrane of the eye, when practised upon those animals where the sympathetic is so closely connected to this nerve, that the one cannot be divided without the other. I had several opportunities of witnessing these on dogs. At a longer or shorter period, after the trunks of the *par vagum* and the accompanying sympathetic nerves were divided, the conjunctiva became red, swollen, and projected over the cornea. The pupil was contracted, and only a small part of the ball of the eye was seen between the half-closed eyelids. This inflammation frequently went on to the secretion of purulent matter, and after lasting some time began gradually to abate. Petit was the first who observed these effects upon the eye after the section of the *par vagum*, and justly attributed them to the division of the trunk of the sympathetic; for he was perfectly aware of the connexion of this nerve with the sixth pair and first branch of the fifth pair within the cavernous sinus, and of the intimate relation of the trunk of this nerve with that of the *par vagum* in the neck in quadrupeds.[1] Cruickshank also noticed this inflammation of the conjunctiva in his experiments upon the *par vagum*. In the first of these it is mentioned that there was "heaviness and slight inflammation of the eye," and in Experiments 2d and 4th, "the eyes," we are told, "became instantly red and heavy."[2]

That Petit was right in supposing this inflammation of

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[1] Mémoire dans lequel il est démontré que les Nerfs Intercostaux fournissent des rameaux, qui se portent des esprits dans les Yeux; dans l'Histoire de l'Académie Royale des Sciences, Année 1727.

[2] Philosophical Transactions 1795, part 1st, or Medical Tracts and Observations, vol. vii., p. 136.

the eye to arise from section of the sympathetic and not of the *par vagum*, has been fully demonstrated by the experiments of Dupuy,[1] upon the effects of the removal of the superior cervical ganglion of the sympathetic. These experiments have been more lately confirmed by Brachet.[2] This inflammation of the eye frequently takes place with great rapidity after section of the sympathetic. In one case, I observed the conjunctiva reddened a very few minutes after the operation. In two of Petit's experiments it is mentioned that in a quarter of an hour after the section of the nerves, the cartilaginous membrane, at the inferior angle of the eye, had encroached upon the cornea. In the fourth experiment by Dupuy upon the horse, it is stated, "Aussitôt après l'opération," the eyelids were swelled, and the eyes watery. This inflammation appears to be confined to the conjunctiva—the contracted pupil and half-closed eyelids probably depending upon the impatience of light generally accompanying this condition. Petit mentions that he killed a dog on the third day after the operation, and on dissection found the inflammation apparently restricted to the conjunctiva. We do not, however, consider it fairly ascertained that the inflammation is confined merely to the surface of the eye. In the experiments of Dupuy and Brachet upon the effects of the removal of the superior ganglion of the sympathetic, to which I have referred, the same phenomena presented themselves, as far as the eye was concerned, as when the *par vagum* is cut in the neck. This inflammation of the conjunctiva from section of the sympathetic in the neck, cannot, in all probability, be referred to the same cause as that produced by section or disease of the fifth pair—the former occurring

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[1] Journal de Médecine, Chirurgie, &c., Décembre 1816, tom. xxxvii., p. 340.

[2] Fonctions du Système Nerveux Ganglionaire, chap. ix. 1830.

almost instantaneously, without arrestment of the usual secretion, and apparently from some direct effect upon the blood-vessels or their contents; the latter coming on more slowly, and apparently arising, as has been ingeniously suggested, from the arrestments of the usual secretions which protect the conjunctiva from the irritating effects of the external atmosphere, as seen in various cases when the nerves of secreting surfaces are cut.[1]

Pharyngeal Branches of Par Vagum.—In the human species we not unfrequently find two pharyngeal branches of the *par vagum*, the lower and smaller of which, as Wrisberg describes, is composed of a filament of the *par vagum*, conjoined with others from the sympathetic. In the dog (the animal upon which the following experiments were made) there is only one pharyngeal branch of the *par vagum* on each side as far as I have observed, and this is composed, as in the human species, of a twig from the internal branch of the spinal accessory, united with another twig from the *par vagum*.

These branches of the *par vagum* have not, as far as I am aware, been previously made the subject of experimental investigation, so that their exact functions have hitherto only been a matter of conjecture.

Is the pharyngeal branch of the *par vagum* both a *motor* and *sensitive* nerve? We have adduced sufficient evidence when detailing the experiments upon the glosso-pharyngeal, to prove that this is a motor nerve, for in repeated experiments made upon animals immediately after death, its mechanical and chemical irritation produced distinct convulsive movements of the muscles in which it is ramified. It is also there stated that the movements seen on irritating this nerve immediately after death are generally very vigorous,

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[1] Alison's Outlines of Physiology, p. 148. 1836.

and embrace not only the *constrictors* of the pharynx and *stylo-pharyngeus*, but also the muscles of the soft palate. These facts are of themselves sufficient to entitle us to conclude that this is a motor nerve of these muscles; and if we are correct in inferring that the glosso-pharyngeal is entirely a nerve of sensation, we may proceed still farther in our inductions, and affirm that this is the principal, if not the sole motor nerve of these parts.[1] In experimenting upon this nerve in the living animal, it is best exposed in the manner described for displaying the glosso-pharyngeal. I find that I have notes of observations made upon the effects of pricking, cutting, and tying these nerves in seven dogs. In four of these it is expressly stated that there were not the slighest indications of suffering; in two, that there were no decided indications of suffering; and that in one there was undoubted evidence of suffering, when these nerves were irritated in the manner mentioned. In all the seven animals, with the exception of the last, the difference between the results of pinching this nerve and the glosso-pharyngeal was very marked. It is quite possible that if this animal, instead of being kept alive for further observation, had been killed at the time, and the nerves carefully dissected, some unusual arrangements of the nervous twigs might have accounted for this difference; for in the other six the nerves were as I have stated, pricked, cut, and tied, and yet no decided evidence of the excitation of pain showed itself. I also distinctly remember, though I have made no mention of it in my notes taken at the time, that we pricked the trunk of this nerve, or its large descending branch in

[1] It is perhaps going too far to say that it is the sole motor nerve of these parts; for Palletta, in describing the smaller portion of the fifth pair, states, that a twig from the external pterygoid branch passes to the *circumflexus palati* muscle. De Nervis Crotaphitico et Buccinatorio. In Ludwig. Scrip. Nov. Min. Se. Tom. iii. p. 74. Mayo also mentions this twig (No. ii. Anat. and Physiol. Comment. p. 8.)

some cases, where these were exposed in experimenting on the glosso-pharyngeal, without causing pain. When we add to all this the smallness of the nerve, compared with the extent of the muscles moved by it, we are led to believe that the sensitive filaments contained in this nerve must be very few, if under ordinary circumstances there are any present at all. I may state that, in one of the animals, in which the constrictors of the pharynx were more freely exposed than usual in operating on the living animal, vigorous contractions were observed in these muscles, when the nerve was pricked with the forceps. I need not state, that to insure accuracy in such experiments, care must be taken that they be made upon the pharyngeal branch of the *par vagum*, before it has received any communicating filament (if such be present) from the glosso-pharyngeal, and that the descending filaments of the glosso-pharyngeal, which cross this nerve, be excluded.

Effects of Section of the Pharyngeal Branch of the Par Vagum upon the Function of Deglutition.—If this nerve be, as we have supposed, the motor nerve of the muscles of the pharynx and isthmus of the fauces, the section of it ought to be followed by considerable derangement of this function. To test this opinion, the pharyngeal branch of the *par vagum* was cut across on both sides, and a portion of it removed, in five dogs. On three of these, satisfactory observations were made. In all the three, the function of deglutition was considerably impaired, and this was manifested exactly in the same manner. I shall briefly detail one only of these experiments.

17*th Exper.*—The *pharyngeal branch* of the *par vagum* was cut on both sides before it had given off any branches. The inflammation and swelling of the neck were allowed to subside before any observations were noted down. This animal, on swallowing a morsel of moderate size, could convey it readily into the posterior part of the mouth; but

at this stage of the process of deglutition it began to make strong movements of the muscles of the neck, during each of which the head was carried down towards the thorax. After a greater or less number of these efforts, the animal again looked out for a fresh portion of food. When the morsel was very small, one or two of these movements were generally sufficient to pass it through the *pharynx;* if large, the movements became more violent, numerous, and prolonged.

It appears, then, that when these *pharyngeal* branches of the *par vagum* are divided, and the *pharyngeal* and *palatine muscles* paralyzed, the food is forced through the pharynx to the commencement of the œsophagus, by the powerful contraction of the muscles of the tongue, and those attached to the larynx and hyoid bone, all of which are moved, except the *digastric* and *stylo-hyoid* muscles, by the *hypoglossal* and descending branches of the *cervical plexus* of nerves. When the morsel is small, the movements of these muscles seem to force it pretty readily (as can be easily imagined) through the bag of the *pharynx*, to the upper part of the *œsophagus*, when drawn forwards and dilated by the ascent of the *hyoid bone.* The difficulty must obviously increase as the size of the morsel increases; and the most violent efforts of the muscles which move the *hyoid* bone and *larynx* are necessary, if they succeed at all, in forcing a large mass through the *pharynx.*

While, then, the glosso-pharyngeal is one of the nerves upon which the sensations of the pharynx and fauces depend, the pharyngeal nerve of the *par vagum* is a motor nerve of the same parts. This view of the functions of these nerves is supported by their ultimate distribution upon the pharynx and fauces, as far as I have been able to trace it. From the free anastomoses of the filaments of the glosso-pharyngeal, pharyngeal branches of the *par vagum*, and the sympathetic, forming what is properly

called the *pharyngeal plexus*, it is absolutely impossible to trace all, or even the greater part of these filaments to their ultimate distribution, unmixed with each other. But, from several minute dissections of these nerves on the human subject, and also upon the dog, where the ramifications of these nerves are fewer and larger, and do not appear to anastomose so frequently as in the human species, I have satisfied myself that those filaments of the pharyngeal branch of the *par vagum*, which do not anastomose with others from the glosso-pharyngeal, are entirely ramified in the muscular fibre; while on tracing the unmixed filaments of the glosso-pharyngeal, they generally, after a long and winding course through the muscular fibres, pass ultimately to the mucous membrane, so that comparatively very few of these are lost in the muscular fibres.

That a few of the filaments of the glosso-pharyngeal appear to be lost in the muscular fibre, is certainly no proof that this is partly a motor nerve; for as it has been shown by Sir C. Bell, and the fact is easily verified, the muscular fibres belonging to the animal functions (and the palatine and pharyngeal muscles are as it were on the debatable ground between the animal and organic functions) are supplied with sensitive as well as with motor filaments, to endow them with the muscular sense. If these attempts to unravel the functions of the most important of the complicated nerves of the pharynx and fauces, and to ascertain their relative shares in the performance of the function of deglutition, be still imperfect, I may be excused on account of the intricacy of the subject, and particularly when it has been stated on the best authority, that, up to this period, "the precise office of each nerve in these parts has not been ascertained."[1]

[1] Alison's Outlines of Physiology, p. 213. 1836.

Laryngeal Branches of Par Vagum.—Before proceeding to the consideration of the experiments upon the laryngeal branches of the *par vagum*, we shall advert for a little to their anatomical distribution upon the larynx, since this bears in a direct manner upon some of the most important questions connected with the functions of these nerves.

After it had been demonstrated by the experiments of Legallois, that compression or section of the *inferior laryngeal nerves*, or of the trunks of the *pneumogastrics* above the origin of these branches, arrested the movements of the muscles of the *glottis*, and were frequently followed by dyspnœa and even by suffocation, particularly in young animals; and when it had also been ascertained by the observations of physicians, that derangement of the movements of these muscles is also not unfrequently the cause of alarming paroxysms of *dyspnœa*, more especially in children, occasionally terminating in death, it became an object of considerable practical importance to ascertain the relative share these nerves have in regulating the movements by which the aperture of the *glottis* is diminished or enlarged. Without a correct knowledge of the manner in which these movements are produced in the healthy state, we cannot with safety advance one step in the explanation of their deranged conditions.

There are obviously two methods of examining this question, viz., by tracing these nerves to their ultimate distribution, and by experiments on animals. If in following the first method, we find that one of these nerves is distributed upon certain muscles of the *larynx*, and not upon others, we must of course consider it as finally ascertained that this nerve has no effect, in regulating, as a motor nerve, the movements of those muscles, upon which it is not distributed. And, as is well known, the anatomical arrangement of these nerves is the strongest fact adduced by Magendie in favour of his opinion, that the *superior*

laryngeals move the muscles which shut the superior aperture of the *larynx*, and the *inferior laryngeals* or *recurrents* those which open it. For, according to Magendie,[1] Cloquet,[2] and many other anatomists and physiologists who have taken it on their authority, the *arytenoid muscles* receive their nervous filaments solely from the *superior laryngeals*. That filaments of the *superior laryngeal nerves* pass into the *arytenoid muscles* is allowed by all anatomists; but there can be as little doubt that they receive a filament from each of the *inferior laryngeals*. Among those who have described this *arytenoid* branch of the *inferior laryngeal or recurrent*, previous to the announcement of this statement by Magendie, we may mention Andersch,[3] Bichat,[4] and J. F. Meckel.[5] And, among those who have since that time examined these nerves, and ascertained the existence of this *arytenoid* branch, we may mention Rudolphi,[6] Bischoff,[7] Swan,[8] and Cruveilhier.[9] I have repeatedly satisfied myself of the existence of this *arytenoid* branch of the *inferior laryngeal*, and the dissection is one which can leave no kind of doubt on the matter. It is obvious that those who have failed in detecting this branch have been misled by the circumstance, that it appears to enter the *crico-arytenoideus posticus*, and to be destined for that muscle. On being traced upwards, however, it is found to continue

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[1] Compendium of Physiology, 4th edit., p. 132. Milligan's translation.

[2] Traité d'Anatomie Descriptive, tom. ii., p. 622.

[3] Fragmentum Descr. Nerv. Card., &c., in tom. ii., p. 139, Ludwig. Scrip. Nerv. Min. Sel. Lipsiae, 1792.

[4] Traité d'Anatomie Descriptive, tom. iii., p. 216. 1819.

[5] Manuel d'Anatomie Gén. Descrip., &c., tom. iii., p. 66. Paris, 1825.

[6] Physiologie, Bd. ii., S. 374. These dissections were made by Schlemm.

[7] Comment. De Nervi Accessorii Willisii Anat. et Physiol., p. 27. 1832.

[8] A Demonstration of the Nerves of the Human Body. Plate xvi., fig. vi., vii. These contain a very accurate representation of the course of this twig.

[9] Anatomie Descriptive, tom. iv., p. 963. 1836. Dr. Sharpey informs me that he has been accustomed to describe this branch in his lectures.

its course inwards and upwards, between the anterior surface of the *crico-arytenoideus posticus* and the posterior surface of the *cricoid cartilage*, to reach the lower margin of the *arytenoid muscles*. I shall shortly state the conclusions I have come to regarding the ultimate distribution of these nerves upon the *larynx* and upper part of the *trachea*, drawn from careful dissections in the human subject. The *recurrent* in its course upwards to the *larynx* sends various filaments to the muscular fibres which complete the tube of the trachea behind, and others which perforate the narrow intervals between the *cartilages* to reach the mucous membrane of the *trachea*. Having arrived at the *larynx*, it sends distinct branches to the *crico-arytenoideus posticus*, *crico-arytenoideus lateralis*, *thyro-arytenoideus* and *arytenoid muscles*, or, in other words, to all the muscles which move the arytenoid cartilages.[1] These branches evidently terminate in the muscular fibre. In fact, the only filaments of the *inferior laryngeal* which appear to proceed to the mucous surface of the *larynx*, are a few from the terminating or *thyro-arytenoid branch*. I have seen one or two very slender filaments pass to the *crico-thyroid*, from that branch of the *inferior laryngeal*, which, after sending ramifications to the mucous surface of the lower part of the *pharynx*, anastomoses with the *external laryngeal* branch of the *superior laryngeal*, but these filaments do not appear to be constant.

Superior Laryngeal Nerve.—The *external laryngeal* branch of the *superior laryngeal* nerve gives a few filaments to the *inferior constrictor of the pharynx*, more to the *thyro-hyoid muscle*, and a comparatively large and distinct branch to the *crico-thyroid*, which evidently terminates in that muscle. The internal laryngeal branch of the same nerve is almost entirely distributed upon the mucous surface of the *epi-*

[1] I here consider the muscular fibres described as the *thyro-epiglottideus* as a part of the *thyro-arytenoideus*.

glottis and interior of the *larynx*. By far the greater part of the filaments of this branch of the nerve which ramify in the *arytenoid*, *thyro-arytenoid*, and *crico-arytenoideus lateralis* muscles, proceed to the mucous surface, and generally, after a long and winding course among the muscular fibres. They thus present a striking contrast to the abrupt manner in which the filaments of the crico-thyroid twig of the external laryngeal terminate in the muscular fibre. In fact, the only filaments of this *internal branch* of the *superior laryngeal* which appear to terminate in the muscular fibre, are some of those which pass into the *arytenoid muscles*. Those filaments which seem to terminate in the arytenoid are part of a twig which anastomoses with the arytenoid branch of the inferior laryngeal in the substance of the arytenoid muscle. A knowledge of the distribution of these nerves is, however, of itself insufficient to clear up their functions in a satisfactory manner, for it must be obvious, that though by anatomical investigation, we may ascertain that certain nerves supply particular muscles, and in this manner frequently form a pretty accurate notion of their function, yet when nervous filaments come from more than one source, and from a complex nerve like the *par vagum*, it is only by experiments upon animals, or by the observation of disease, that we can hope to ascertain which are motor and which are sensitive. Besides the free anastomoses between those nerves must render it doubtful whence some of the minute filaments come.[1]

In entering upon the physiological investigation of the functions of the *laryngeal nerves*, I first proceeded to examine what effect the irritation of these has upon the muscles of the *larynx* in a recently killed animal.

18*th Exper.*—The *larynx* was exposed and the *glottis* brought into view in a dog, immediately after it had been

[1] For some further remarks upon the anatomy of these laryngeal nerves, see Part III.

killed by a dose of prussic acid. On applying the galvanic wires to each recurrent nerve alternately, violent movements of the muscles of the *larynx* followed, and the *arytenoid cartilages* were first seen to approach each other and then to recede. At each movement the small cartilages at the summit of the *arytenoids* (cornicula laryngis) came into close contact. On galvanizing the *superior laryngeal* nerves, or rather the *internal branches* of these, for they had been cut across a little above where they perforate the *thyro-hyoid ligament*, no movement was observed. On again applying the wires to the recurrents, or to the trunk of the *par vagum* above the origin of the *recurrent*, the same results were obtained as before. The movements which followed irritation of the trunk of the *par vagum* were not so strong as those from irritation of the *recurrent* itself. Dr. Alison was present at this experiment.

19*th Exper.*—The *larynx* was exposed, as in the preceding experiment, in a dog bled to death from the femoral arteries, but without dividing the *superior laryngeal nerves.* On applying the galvanic wires to the *superior laryngeals* before they had given off the *external laryngeal* branch, strong convulsive movements of the *crico-thyroid* muscle followed, by which the *cricoid* cartilage was approximated to and drawn under the *thyroid* and the *larynx* shortened. All the muscles attached to the *arytenoid* cartilages remained perfectly quiescent, so that no change took place upon the superior aperture of the larynx. On applying the wires to the *recurrent* nerves alternately, the same vigorous movements of the *arytenoid* cartilages were observed as in the preceding experiment. It was remarked that when the galvanic wires were kept applied to one of these *recurrent* nerves for some short time, the *arytenoid* cartilages were so approximated as to shut completely the superior aperture of the larynx. On removing one of the wires the cartilages then separated.

These experiments were repeated on five other animals with the same results. In two of these the movements observed on irritating the nerves were much feebler than those described, but, though varying in degree, as might be expected, they never varied in kind. I may add, that these movements are also well marked when the nerves are pinched with the forceps, and after they have been detached from the trunk of the *par vagum*. In these experiments it was distinctly observed that the only outward movements of the *arytenoid* cartilages, seen on irritating the *recurrents*, were merely occasioned by their return to their former position after they had been carried inwards. This outward movement, then, no doubt, entirely depended upon the elasticity of the parts.[1]

From these experiments it was concluded that all the muscles which move the *arytenoid* cartilages receive their motor filaments from the *inferior laryngeal* or *recurrent* nerves. And as the force of the muscles which shut the larynx preponderates over that of those which dilate it, so the arytenoid cartilages are carried inwards when all the filaments of one or both of these nerves are irritated. These experiments also show us, that one only of the intrinsic muscles of the larynx receives its motor filaments from the superior laryngeal, viz. *the crico-thyroid* muscle, and that, consequently, the only change which this nerve can produce on the *larynx*, as a motor nerve, is that of approximating the cricoid to the thyroid cartilage—in other words, of shortening the larynx. I find that Bischoff[2] had examined the

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[1] In these experiments I never could perceive any contractions of the *thyro-hyoid* muscle and *inferior constrictor* of the pharynx, on irritating the superior laryngeal, even when the *crico-thyroid* was acting most vigorously. Neither could I ever observe any movements of the *crico-thyroid* on irritating the recurrent. The *thyro-hyoid* muscle receives its motor filaments from the *hypoglossal* and the *inferior constrictor* of the *pharynx* from the *pharyngeal branch* of the *par vagum*.

[2] Oper. cit., p. 27.

effect of irritating these nerves in the recently killed animal. And though he failed to observe the contractions of the *crico-thyroid* muscle, and the consequent shortening of the *larynx*, produced by irritation of the *superior laryngeal* nerve, and in place of this describes some supposed palpitation of the mucous membrane of the *larynx*, yet the other results obtained by him were similar to those we have described in the 19th Experiment.

We have now to see how far the views we have stated above, are supported by the subsequent experiments on living animals.

The *superior laryngeal* nerve was cut on both sides in two dogs and one rabbit, and the animals readily swallowed both solids and fluids without exciting the slightest cough or the least difficulty of breathing. The lungs were carefully examined after death, and none of the food taken could be detected in the air-passages. In several animals the superior laryngeals were first cut, and the inferior laryngeals immediately afterwards, and it was ascertained that the previous division of the *superior laryngeals* did not prevent the difficult breathing and symptoms of suffocation, which not unfrequently follow the division of the *inferior laryngeal* nerves, particularly in young animals. To procure still more positive assurance of the effect of section of the different *laryngeal* nerves upon the movements of the muscles attached to the *arytenoid* cartilages, the following experiments were performed.

20*th Exper.*—All the *four laryngeal* nerves were exposed in a full-grown cat. The larynx was then dissected out by cutting between the hyoid bone and thyroid cartilage, and drawn forward so as to expose the glottis without disturbing the nerves. When the glottis came into view, the *arytenoid* cartilages were observed to be drawn backwards and outwards during inspiration, and to approximate considerably during expiration. The extent of these move-

ments of the muscles of the larynx was in proportion to the extent of the other respiratory movements. When the animal was quiet and breathing less forcibly, the movements were slight. While uttering a cry, the sides of the superior aperture of the larynx were closely approximated, and were thrown into vibratory motion. While struggling violently, the superior aperture of the larynx appeared completely closed. A small opening was made into the trachea, and a silver probe passed upwards. This appeared to excite little if any uneasiness, until it arrived at the larynx. As soon as it entered the larynx it was instantly followed by close approximation of the sides of the superior aperture of the larynx, violent cough, and evident uneasiness. The same effect was produced by introducing the probe from above. After satisfying myself of these facts, one of the *recurrent* nerves was first cut across, with the effect of evidently diminishing the movements of the *arytenoid* cartilage on the side cut. The other *recurrent* was then divided, and instantly all the movements of the muscles of the *glottis* ceased, and the *arytenoid* cartilages were never carried outwards beyond the position in which they are found after death. The *superior laryngeals* were then cut, without effecting the slightest enlargement or any other change upon the *glottis*. As the *arytenoid* cartilages, after section of the *inferior laryngeal* nerves were now mechanically carried inwards by the rushing of the air through the diminished aperture of the *larynx*, during each violent inspiration of the animal, by which the aperture was still further contracted, its edges were kept apart with the forceps, until an opening was made in the *trachea* to prevent the immediate suffocation of the animal.[1] The experiment being now completed, the animal was killed by a dose of prussic acid.

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[1] It is important to remark, that section of the superior laryngeals did not arrest these inward movements of the arytenoid cartilages.

21*st Exper.*—The *larynx* was brought into view in another full-grown cat, as in the preceding experiment, and the various movements of the muscles of the *glottis* again watched for a short time. The *superior laryngeal* nerves were then cut without diminishing in the least any of the movements of the *arytenoid* cartilages. The sides of the superior aperture of the *larynx* were approximated in crying, so as to form but a narrow fissure, and in struggling, the aperture became completely closed, as when the *superior laryngeal* nerves were uninjured.

Nothing could be more satisfactory than the results of these two last experiments, and they complete the accumulated facts which we have adduced, in fully disproving the statement of Magendie, to which we have already referred, viz. that the *inferior laryngeal* supplies those muscles only which enlarge the aperture of the *glottis*, while the *superior laryngeal* furnishes the motor filaments to those muscles which shut the *glottis*.[1] They also illustrate in a very satis-

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[1] Allowing even that the recurrent nerves gave no filaments to the arytenoid muscles, the proposition of Magendie, that the superior laryngeal nerve alone moves those muscles which shut the glottis, would not necessarily be correct. For it is universally admitted, that filaments of the recurrent nerve are distributed in the thyro-arytenoid muscle, and no attempt has been made to prove these filaments to be merely sensitive; on the other hand, we have seen, from the anatomical distribution of these nerves, that this muscle must receive its motor filaments from the recurrent. Now, an examination of the course of the fibres of this muscle will, we think, fully bear out the generally received notion of its action, viz. that it must assist in diminishing the aperture of the glottis. The authority of Hoffman, (Aeroteria, p. 91, as quoted by Haller,) of Haller, (Element. Physiol. tom. iii., p. 387, Laus. 1766,) and Soemmerring, (De Corporis Humani Fabrica, tom. ii., p. 138. 1794,) may doubtless be adduced in support of the opinion, that it enlarges the glottis. We consider, however, this view of the action of the muscle untenable. In confirmation of the opinion, that the action of this muscle has a contrary effect, and assists in diminishing the glottis, we may quote the authority of Cowper, (Myotomia Reformata, p. 46. London, 1724;) Albinus, (Historia Musculorum Hominis, lib. iii., cap. 68. Leidae, 1734;) Winslow, (Anatomical Exposition, &c., translated by Douglas, vol. ii., p. 329. London, 1763;) Meckel, (Oper. cit., tom. iii., p. 497;) and

factory manner the cause of the *dyspnœa* in some cases where the *inferior laryngeal* nerves are cut, compressed, or irritated. To the consideration of this last point we shall afterwards return. The movements of the muscular fibres of the *trachea*, no doubt, depend upon the *recurrent*, but we had no opportunity of actually witnessing this. If it were thought necessary to adduce any additional evidence, that the muscles of the larynx can act involuntarily, like the other muscles of respiration, we might state that we have seen these movements going on in unison with those of the other respiratory muscles, in animals deprived of all volition by a fatal dose of prussic acid.

We have now to examine *to what extent these nerves are connected with sensation*. And first, with regard to the effect of irritating the trunks of these nerves. I have exposed the *superior laryngeal* nerves repeatedly in living animals, and in all, as I have already stated, decided indications of suffering presented themselves, except in two dogs which had previously endured protracted pain. In some of these dogs, distinct convulsive movements of the muscles of the throat and lower part of the face were observed, similar to, but less strongly marked than those accompanying irritation of the glosso-pharyngeal. Whether this movement depended upon the sudden excitation of pain, or of some specific sensation, similar to, though feebler than that we suppose to attend irritation of the *glosso-pharyngeal* nerve, we cannot pretend to determine. In support, however, of this last

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Lauth, (Mém. de l'Acad. Roy. de Médec., tom. iv., p. 110. 1835.) Some authors, as Meckel, Lauth, and Cruveilhier, (Oper. cit., tom. ii., p. 669,) even maintain that the action of the crico-arytenoidei laterales muscles is to diminish the aperture of the glottis. Bichat also asserts, (Anat. Descrip., tom. ii., p. 407,) that "les crico-aryténoïdiens postérieurs sont les seuls agens de la dilatation de la glotte." The action of these crico-arytenoidei laterales muscles is certainly difficult to determine, as it must depend in a great measure upon the position of the *arytenoid* cartilages at the time they are thrown into action, and the synchronous contraction of other muscles.

supposition, we may urge the anatomical fact, that a considerable number of the filaments of this nerve are distributed upon the mucous surface of the *pharynx.* I was anxious to ascertain whether irritation of these nerves would produce closure of the glottis by a reflex action. As the experiment is one in which it is difficult to arrive at accurate conclusions, without inflicting much pain, I did not persevere in the attempt. I may state, however, that in the 21st Experiment we observed, that when each of the superior laryngeal nerves was cut, the glottis was suddenly closed; but whether this was occasioned by a struggle of the animal from the excitation of pain, or depended upon the muscles of the glottis being thrown into contraction by a reflex action, I cannot venture to decide. Irritation of the recurrent nerves is attended by much feebler indications of suffering. That this nerve does, however, contain some sensitive filaments, is indicated not only by the circumstance, that when pinched or tied, the animal generally gives symptoms of feeling pain, but also from the fact, that it sends numerous filaments to the mucous surface of the *trachea,* a few to the mucous surface of the lower part of the pharynx, and even a few to the inner surface of the *larynx.*[1] With regard to the effects of section of these nerves upon the sensibility of the mucous surface of the larynx, we have obtained very satisfactory results. We might *a priori* determine from the anatomical distribution of these nerves, that the sensations referred to the larynx are almost entirely dependent upon the superior laryngeal nerves. In the 20th Experiment we observed that irritation of the mucous surface of the *larynx* after section of the *inferior laryngeals,* was still followed by great uneasiness and efforts to cough, while the movements of all the

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[1] We shall afterwards have occasion to examine the functions of the œsophageal filaments of the recurrent.

muscles of the *glottis* were arrested. In the 21st Experiment, on the other hand, no uneasiness, or efforts to cough were excited even by rubbing the probe against the inner surface of the larynx, after section of the superior laryngeals, while the sympathetic and voluntary movements of the muscles of the glottis went on as before. I have also had occasion, while performing other experiments, to verify the same facts in the manner followed by Magendie in one of his lectures at the Collège de France.[1] Through an opening made into the trachea, a probe was passed upwards to the larynx. This excited little or no uneasiness until it reached the larynx. As soon as it entered the larynx, it excited great uneasiness and violent paroxysms of coughing. Section of the recurrents had no effect in diminishing the severity of these paroxysms of coughing, while they were instantly arrested by cutting across the superior laryngeals. Whether this striking difference between the sensibility of the parts supplied by the two laryngeal nerves depends upon the number of these filaments, and the manner in which they are distributed, or upon some difference in their endowments, I cannot pretend to decide.

In the 20th Experiment we observed that when the *superior laryngeals* were divided, the presence of the probe in the interior of the larynx was not only unattended by uneasiness and cough, but it also no longer occasioned any sudden closure of the superior aperture of the *larynx;* and in the 21st Experiment, though each introduction of the probe occasioned great uneasiness and efforts to cough after section of the *recurrents,* still the muscles of the *glottis* were not thrown into spasmodic contractions, as when they were entire. From these experiments we perceive, that when either the *recurrents* or *superior laryngeals* are divided, so

[1] Lancet, July 1, 1837.

as to break the nervous circle which these form through their connexion with the central organs of the nervous system, irritation of the mucous surface of the *larynx* no longer excites contraction of the surrounding muscles, though their contractility has not been impaired. This nervous circle we may suppose to begin at the mucous surface of the *larynx*, to pass upwards through the filaments of the *superior laryngeals* to the *medulla oblongata*, and back again to the muscles through the filaments of the *recurrents*. I have also repeatedly attempted, but in vain, to excite contractions of the muscles of the *glottis*, by irritating the mucous surface in an animal recently killed. From these facts, we think that we are justified in concluding, that when any irritation is applied to the inner surface of the *larynx* in the healthy state of the parts, this does not excite the contraction of the muscles attached to the *arytenoid* cartilages by acting directly upon them through the mucous membrane, but that this contraction takes place by a reflex action, in the performance of which the *superior laryngeal* is the *incident*, and the *inferior laryngeal* is the *motor nerve*. And when we remember that no appreciable interval of time intervenes between the application of an irritant to the mucous surface of the glottis and the contraction of its closing muscles, and reflect upon the circuitous course through which the nervous influence must travel, before it reaches these muscles to stimulate them to contraction, we may form some faint idea of the astonishing rapidity with which changes are accomplished in the nervous system. We also ascertain from these experiments that in the small *recurrent* nerves two sets of motor filaments are included—one set transmitting the nervous influence which stimulates the opening muscles of the *glottis* to act synchronously with the other muscles of inspiration, the other set transmitting the nervous influence which stimulates the closing muscles to act synchronously

with the other muscles of expiration. And when we remember that these filaments arise near each other, and from the same medullary tract of the spinal cord, this would furnish us with an additional proof (if any more were necessary) of the futility of attempting to explain sympathetic or associated actions by mere nervous contiguity.

From all these experiments, then, upon the laryngeal nerves, we are inclined to draw the following conclusions:

1. That the superior laryngeal furnishes one muscle only with *motor* filaments, viz., the *crico-thyroid.*

2. That the superior laryngeal furnishes all, or at least nearly all, the *sensitive* filaments of the *larynx*, and also some of those distributed upon the *mucous surface* of the pharynx.

3. That the inferior laryngeal or recurrent furnishes the *sensitive* filaments to the upper part of the trachea, a few to the *mucous surface* of the pharynx, and still fewer to the *mucous surface* of the larynx.

4. That when any irritation is applied to the mucous membrane of the larynx in the healthy state, this does not excite the contraction of the muscles that move the arytenoid cartilages by acting *directly* upon these through the mucous membrane, but that this contraction takes place by a *reflex action*, in the performance of which the superior laryngeal is the *incident*, and the inferior laryngeal is the *motor* nerve.[1]

Does Section of the Laryngeal Nerves prevent the ingress of the Food into the Larynx during Deglutition?—All the *four laryngeal nerves* were divided in two dogs and four rabbits. Both dogs and two of the rabbits swallowed solids and fluids readily, and without exciting cough or

[1] The functions of the œsophageal filaments of the *recurrent* will be examined along with the other *œsophageal* filaments of the *par vagum.*

difficulty of breathing. The other two rabbits refused the milk, but they swallowed solids without inconvenience. All these animals were carefully examined after death, and not the slightest trace of the food could be detected in the air-passages. From these experiments it would appear that if the closing muscles of the glottis can, as Magendie has shown, prevent the entrance of food into the larynx after removal of the epiglottis, the epiglottis can, on the other hand, prevent the ingress of food into the larynx, when the movements of all the muscles which diminish the size of the glottis have been suspended by section of the laryngeal nerves.

Effects of Section of the Laryngeal Nerves upon the Voice.—In the experiments upon the two dogs mentioned above, and others which we might adduce, in which the superior laryngeal nerves were cut, I could detect no change upon the voice. I will not by any means maintain that no change was effected, for my perception of differences in sound is far from being good. Since the variations in the length of a tube alter the graveness and acuteness of the sounds which it emits, we would expect that section of the *superior laryngeal* should, by arresting the movements of the *crico-thyroid* muscles, produce some change in this respect. Magendie[1] mentions that an animal, after section of the *superior laryngeal* nerves, " loses almost all its acute sounds; it acquires besides a constant gravity, which it had not previously." This he attributes to the arrestment of the movements of the *arytenoid* muscles. But we have shown that section of these nerves has no such effect. Supposing Magendie quite correct[2] in the occurrence of

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[1] Oper. cit., p. 138.

[2] Bischoff (Oper. cit., p. 27,) says, " duobus canibus *laryngeum superiorem* utrimque dissecui; sed neutrius canis vox nec post plures quidem dies mutata est."

this change of the voice, may it not be explained in this manner. He himself states,[1] that during acute sounds the vocal tube is shortened, and lengthened during the formation of those which are grave. Now, section of the superior laryngeals would necessarily have the effect of preventing the tube from being so much shortened as during the natural action of the parts; in other words, it might prevent the production of the acute sounds; and may not the relaxed state of the *crico-thyroid* muscles be one of the conditions necessary for the production of a grave sound?

With regard to the effects of section of the recurrent upon the voice—a physiological experiment which has been performed so very frequently since the time of Galen, we have no observations to make. We may merely remark, that we found, as the second Monro[2] has stated, that the voice is not altogether lost, for we ascertained that the animal, in some cases at least, could still emit a very faint howl.

It is not my object here to examine the various combined contractions of the muscles of the larynx, which these laryngeal nerves effect—whether instinctively or sympathetically, as in natural or inarticulate language, and in the function of respiration; or voluntarily, as in artificial or articulate language. Of the great disturbance of these important functions, which either necessarily or occasionally arises from section of these nerves, we have had abundant proof. Perhaps the most interesting of all the associated movements of these muscles to the practical physician, are those connected with the respiratory function, as they promise to throw light upon some important forms of disease. Since these movements were first pointed out by

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[1] Oper. cit., p. 139.

[2] Observations on the Nervous System, p. 65. 1783.

Legallois,[1] they have been repeatedly observed in the lower animals by several experimenters; and by Mr. Mayo[2] and Sir C. Bell,[3] in individuals of the human species, after unsuccessful attempts at suicide. The energy and extent of these movements, when the respiration is much hurried by the struggles and pain of the animal, appear quite astonishing when witnessed for the first time. When the respiration, however, is such as is natural in a state of rest, the sides of the glottis remain quiescent, or at least nearly so.[4] The manner in which section or compression of the laryngeal nerves arrests these movements is well illustrated by the 20th and 21st Experiments. From these we learn, that when the inferior laryngeal nerves are cut, all the movements of the muscles of the arytenoid cartilages are arrested, and the superior aperture of the glottis can no longer be dilated during inspiration. In fact, the sides of the *glottis* are not only no longer separated by an active force, but they are rendered quite passive, and yield readily, within certain limits, to the slightest external force applied to them. The *glottis* now presents the appearance which it does some time after death; indeed it is probably still more diminished in size, for as the contractility of the muscular fibre is unimpaired by section of the nerves, their tonicity may still occasion further diminution, since the strength of the closing, preponderates over that of the opening muscles. When the *recurrent* nerves are cut in an adult animal, where the aperture of the *larynx* is large, a quantity of air may still find its way through the diminished aperture of the *glottis*, adequate in many cases to

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[1] Sur le Principe de la Vie, pp. 185-202. 1812.

[2] Medical Gazette, vol. xiv., p. 22. 1834.

[3] Nervous System, p. 484, ed. 3, or Phil. Trans. 1832.

[4] Legallois and Mayo, oper. cit. It is probable that when the larynx is small, as in young animals, these movements are constant, even when the respiration is not hurried.

carry on the respiratory process in a sufficient manner, particularly if the muscles of inspiration are not acting violently. If, on the other hand, the capacity of the *larynx* is proportionally smaller, as in young animals, the air rushes through the diminished aperture in a narrower stream and with increased force, more especially when the inspiratory movements are violent—or, in other words, when the capacity of the thorax is suddenly and greatly enlarged—and the consequence is, that an insufficient quantity of air reaches the lungs. This quantity is still further reduced, by the circumstance, that the now passive sides of the superior aperture of the larynx are carried inwards by the current of air, as in the 20th Experiment, where it was remarked that, at each inspiration, the arytenoid cartilages were so closely approximated, as almost entirely to prevent the ingress of air, and where it was necessary to hold aside the edges of the superior aperture of the *larynx*, to prevent the immediate suffocation of the animal. It is the inspiration alone of the animal which is difficult, for the expiration is easy. The *arytenoid* cartilages are readily pushed aside by the expired air. The occurrence or non-occurrence of dyspnœa or suffocation after section of the *inferior laryngeals* is to be explained by the greater or less capacity of the larynx in the individual animal, and the violence of its respiratory movements at the time. The crowing sound which frequently attends this condition of the laryngeal muscles, is of course a mere physical effect, and depends upon the current of air rushing rapidly through the diminished aperture of the glottis, and may be imitated in the larynx of a dead animal. It is evident, then, that we must again return to Legallois' explanation of the cause of the *dyspnœa* after section of the *recurrent* nerves, which I have been here illustrating and extending. For though this acute physiologist does not enter upon the consideration of the relative share which these nerves have in the move-

ments of the *intrinsic muscles* of the larynx, yet he distinctly attributed the dyspnœa after the section of the recurrents to muscular paralysis.[1] He also explained the manner in which the arytenoid cartilages are carried inwards by the current of air passing through the *glottis*, from what takes place when a syringe is fixed to the trachea—detached with the larynx from the body after death—and the air drawn forcibly downwards. At each stroke of the syringe, the edges of the superior aperture of the glottis are approximated, and if the current of air be strong, the aperture is completely shut.[2]

Whether the crowing disease of children, or *laryngismus stridulus*, depends upon the movements of the muscles of the glottis being suspended by compression of the *recurrents* or of the *pneumogastrics* themselves above the origin of these branches, by enlarged glands, as the late Dr. Ley supposed, or whether, as is more generally imagined, it depends upon a *spasm* of these muscles, and "is obviously a part of a more general spasmodic affection,"[3] it is not for me to determine. From the experiments we have detailed it is, however, apparent, that severe dyspnœa amounting to suffocation may arise both from irritation and compression

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[1] "C'est donc bien réellement en paralysant les muscles aryténoïdiens et en relâchant par là les ligaments de la glotte, que la section des nerfs recurrens produit la suffocation." Whether Legallois by the term arytenoid muscles, here means the muscles universally known by this name, or whether he erroneously extends it to all the muscles attached to the arytenoid cartilages, I have not been able to ascertain from his writings.

[2] In performing this experiment, care must be taken to produce a large and rapid current of air, such as we would suppose to pass through the larynx, when the chest is rapidly and fully dilated in a violent inspiration. If the syringe employed is one of moderate size, a small larynx must be selected, otherwise the experiment will fail.

For an excellent exposition of the principal conditions of the system under which violent inspirations are apt to be made, or, in other words, when the aperture of the glottis is likely to be most diminished, see Dr. Ley on Laryngismus Stridulus, p. 85.

[3] Dr. Marshall Hall's Lectures on the Nervous System, p. 76. 1836.

of the inferior laryngeal nerves, or the trunks of the *pneumogastrics.* For when both, or even one *recurrent* nerve was irritated, the *arytenoid* cartilages were approximated so as in some cases to shut completely the superior aperture of the *glottis;* and we have already explained at some length how paralysis of this nerve, occasioned by compression or any other cause, should produce this effect by arresting all the movements of the muscles of the *glottis.* We shall leave it to those who have had frequent opportunities of seeing this disease, to determine which of these two causes will best explain the phenomena which it presents. There appears, however, to be little doubt, that the crowing respiration and dyspnœa which accompany some cases of hysteria, depend upon a spasmodic closure of the *glottis,* produced by some irritation of the *recurrent* nerves.

Œsophageal Branches of Par Vagum.—It is obvious, that as the *œsophageal* branches of the *par vagum* cannot be divided separately, we can only ascertain the functions of these by cutting the trunk of the *par vagum* itself in the neck.

Effects of the irritation of these nerves in an animal recently killed.—I have repeatedly seen violent muscular contractions induced along the whole length of the tube of the *œsophagus,* by mechanical or chemical irritation of the trunk of the *par vagum* in *dogs, rabbits,* and *cats.* At each application of the irritant, the *œsophagus* became shortened and diminished in calibre. These movements extended also to the *cardiac* extremity of the stomach. In the stomach they were evidently more slow, prolonged, and vermicular, than in the *œsophagus.* They extended somewhat slowly from the cardiac orifice, over a greater or less extent of the left portion of the stomach. In one case they were more extensive and rapid than in the others, but still retained somewhat of their vermicular appearance. Muscular contractions of the œsophagus on irritating the *par vagum* in an

animal recently killed, have been observed by Arnemann,[1] Cruikshank,[2] and Mayo.[3] Mr. Mayo could not perceive any muscular movements of the stomach upon irritating this nerve. These were, however, frequently observed by Tiedemann and L. Gmelin.[4] They have also been inferred by Breschet and Dr. M. Edwards,[5] from the effects of galvanizing the lower ends of the cut *pneumogastrics* in the neck on the living animal.

Effects of section of the œsophageal branches upon the movements of the œsophagus.

22d Exper.—A portion of the *par vagum* was removed on both sides, high in the neck, in a rabbit, after it had fasted sixteen hours. The portions removed included the *second* ganglion of the *par vagum*, and consequently the origin of the *superior laryngeals.* The respiration continued heaving, and somewhat difficult for a short time, but it soon became pretty easy. Some parsley was now thrown down on the opposite side of the room, which it immediately made for· and began to eat. It appeared to swallow the first mouthfuls readily, and without inconvenience. As it continued to eat, its breathing again became heaving, and it apparently felt very uneasy. It was soon after seized with attempts to vomit or cough,[6] and retired from its food. After a short

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[1] As quoted by Soemmerring, De Corporis Humani Fabrica, tom. iv., p. 272. 1794.

[2] Medical Facts and Observations, tom. vii. p. 153, or Phil. Trans., 1795.

[3] Mayo, Anatomical and Physiological Commentaries, No. ii., p. 15.

[4] Recher. Expér. Physiol. et Chéim. sur la Digestion, &c. Première Partie, p. 374. Paris, 1826.

[5] Archiv. Gén. de Méd., tom. vii., p. 187. 1825.

[6] It certainly presented much more the appearance of a cough than an effort to vomit. As, however, apparently similar muscular movements have been described by late experimenters, as an effort to vomit, and the possibility of the excitation of the sensation which induces coughing, under such circumstances, has been strongly denied, I will not at present venture an opinion on this question, until I have examined it more thoroughly. We shall call it in the meantime an effort to vomit. We could not expect a perfect cough in an experiment of this kind, after the movements of the muscles of the glottis had been arrested.

time the *dyspnœa* abated somewhat, and it returned to eat. It had only swallowed a small quantity when the uneasiness, *dyspnœa*, and efforts to vomit, became as urgent as before. Every time it returned to eat (and it did this several times) these were always much increased in severity, so that it more than once appeared dying from suffocation. The breathing was now constantly very difficult, with a rattling sound in the *trachea*. Five hours after the section of the nerves, it again approached the parsley, but it had only eaten a very short time when it sprung into the air, and after making violent struggles, and apparently suffering great uneasiness for nearly a minute, it died.—*Dissection.* This was done immediately after death. The nerves were found fairly divided. On exposing the trachea in the neck, I was struck with the great size of the œsophagus, which was distended like a sausage, and evidently compressed the trachea. The green colour of the parsley was quite distinctly seen through the coats of the œsophagus. The pharynx contained some parsley, but was by no means filled by it. The œsophagus in the thorax was distended in the same manner as in the neck, down to the stomach. The stomach was well filled, but was certainly far from being unnaturally distended, and could have easily held more in its pyloric extremity. Considerable quantities of the masticated parsley were found in the larynx, trachea, and bronchi. In some parts of the lungs it was found in the minutest air-cells, and one or two portions were perfectly dense, from the quantity which they contained.

23*d Exper.*—The *pneumogastrics* were cut above the origin of the *superior laryngeals* in another full-grown rabbit, after it had fasted twenty-four hours. This was followed by pretty severe dyspnœa and uneasiness. It refused for some time to eat the parsley laid before it. When it began to eat, it was observed that it swallowed the first mouthfuls perfectly, and without any additional uneasiness. As it

continued to eat, it became restless, and was affected similarly to the animal in the preceding experiment. The breathing after this continued very difficult, and it made but very few subsequent attempts to take food. It was seen alive ten hours after the section of the nerves. It was found dead next morning.—*Dissection.* The *œsophagus* was distended by the parsley, though not to the same extent as in the preceding experiment. The calibre of the *trachea* was also evidently somewhat diminished by the stuffed *œsophagus.* The stomach contained a moderate quantity of food, but was by no means fully distended. A small quantity only of the masticated parsley was found in the air-tubes.

We believe that in these two experiments the first mouthful was carried into the upper portion of the *œsophagus* in the usual manner, but as the muscular movements of the tube had been suspended, it remained there. As additional mouthfuls arrived they propelled forwards those which preceded them, so that after a while these formed a column of food reaching from the lower part of the pharynx to the stomach. As the difficulty of propelling it into the stomach increased, the œsophagus became more distended, and pressed upon the trachea, and thus produced the *dyspnœa* and uneasiness. The difficulty of propelling the food still increasing, it accumulated in the *pharynx*, and passed into the open *larynx;* hence the severe paroxysms of *dyspnœa* and suffocation in the 22d Experiment. Its passage into the *larynx* was, in the two experiments we have just related, much facilitated by the section of the vagi above the origin of the *laryngeal* nerves. It is important to observe, that in these experiments, the stomach was found after death to contain only a moderate quantity of food, notwithstanding the great distention of the *œsophagus.*

This distention of the *œsophagus* after section of the *par vagum* in the neck, has been remarked by several observers.

Some of these have contented themselves with the simple statement of the fact; others have attributed it to paralysis of the *œsophagus;* while others again have referred it to other causes. Baglivi in one experiment found the œsophagus distended through its whole length, and though he does not state that he attributed this to paralysis of the œsophagus, yet it is probable that this was his opinion ; for after stating the circumstance, he adds, "nam cibus in ventriculum descendere non poterat."[1] Valsalva has also observed this retention of the food in the œsophagus, and it is likewise probable that he believed that it depended upon the same cause; for after mentioning that he dissected a dog which died on the tenth day after section of these nerves, and had vomited frequently, he states, "quidquid autem cibi postremis diebus retinuerat, id omne intra œsophagum ab hujus initio ad sinistrum usque orificium ventriculi, nulla ejus facta mutatione, continebatur."[2] Dupuy distinctly refers this retention of the food in the œsophagus to its paralysis.[3] In several experiments upon the effects of section of the *pneumogastrics* in horses, he ascertained that the œsophagus was distended, while the animals were still alive. That the œsophagus is really paralyzed, it is easy, he says, to convince ourselves, "en mettant l'œsophage à découvert. Si on fait boire l'animal, on n'apperçoit aucun mouvement dans la membrane charnue de l'œsophage," &c. Sir A. Cooper[4] in his late experiments on the *par vagum*, after mentioning this retention of the food in the œsophagus, also refers it to paralysis of that tube. Several experimenters, however, explain this distention of

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[1] Opera omnia, p. 676. Antvverpiæ, 1715.

[2] Valsalvæ Opera cum Epistolis Anatomicis, &c. J. B. Morgagni, Epist. Anatom. xiii. 37. Venet. 1740.

[3] Journal de Médecine, Chirurgie, &c., tom. xxxvii., p. 351

[4] Guy's Hospital Reports, Sept. 1836.

the œsophagus differently. The late Mr. Broughton maintained, that "the parsley found in the œsophagus must be the result of ineffectual efforts to throw it off the stomach."[1] Dr. Wilson Philip also maintains the same opinion.[2] Brachet seems to think that the œsophagus becomes filled in consequence of the animal having lost the sense of satiety, and continuing to eat after the stomach has been distended to the full.[3] In another place he seems to admit that the lower part of the œsophagus may be paralyzed.[4] Some others seem to think that since the cardiac orifice of the stomach is paralyzed, the food is readily forced from the stomach into the œsophagus. On reviewing the 22d and 23d Experiments, and those which we have referred to above, it is at once apparent, that they are not sufficient, with the exception, perhaps, of those of Dupuy, fully to invalidate the explanation in which it is maintained, that the distention of the œsophagus depends upon the food being forced back from the stomach. Both of the animals we experimented on made very frequent efforts to vomit after eating. It was, therefore, necessary to perform the experiment in another way.

24*th Exper.*—The *pneumogastrics* were cut in the neck, as in the two preceding experiments, in two full-grown rabbits, which had been made to fast for twenty-six hours. Though the breathing of these animals was not difficult, it continued to be evidently somewhat heaving. About an hour after the section of the nerves, a quantity of parsley was offered to one of them, and it began immediately to eat. The first mouthfuls were swallowed without any change upon the breathing. Gradually, however, the breathing became more hurried and laboured as it continued to eat,

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[1] Quarterly Journal of Science, &c., vol. x., p. 313. 1821.
[2] Experimental Inquiry, &c., pp. 112, 113. Edit. 3d.
[3] Système Nerveux Ganglionaire, &c., pp. 180, 181. 1830.
[4] Oper. cit., p. 204.

and when the dyspnœa had become pretty urgent, it was instantly deprived of life by a blow upon the head, and before it had given the slightest indications of any effort to vomit.—*Dissection.* This was performed immediately after death. The *œsophagus* was found filled throughout its whole length with parsley, and it had evidently diminished the calibre of the *trachea.* The *stomach* contained but a very moderate quantity of food, and this when examined was found to consist principally of substances taken the previous day. In fact, a quantity of parsley, not exceeding a few leaves, was all that had reached the stomach, and was quite easily distinguished from the old food. This was lying between the outer surface of the old food and the inner surface of the *cardiac* extremity of the *stomach.* Not the slightest traces of the parsley could be found in any of the air-passages. On first exposing the distended *œsophagus,* it was observed that irritation of the *lower* end of the cut *pneumogastrics,* both by the forceps and galvanism, produced vigorous contractions of the muscular fibres of the *œsophagus,* which extended over the *cardiac* portion of the *stomach.* A similar experiment was performed on the second rabbit, about two hours after section of the nerves, with exactly the same results. The same movements were also observed in the œsophagus and stomach, on irritating the lower end of the cut nerves. Dr. Alison witnessed the dissection of the first rabbit, and the experiment on and the dissection of the second.

We believe that these experiments prove quite satisfactorily, that the *œsophagus* is distended before the *stomach,* and also fully bear out the explanation given above, of the phenomena arising from the retention of the food in this tube. It must be remembered that when the *pneumogastrics* are cut in the neck, even as high as the origin of the *superior laryngeals,* that the pharynx, and a very small portion of the œsophagus next to it, still retain their healthy

action, for, as we have already seen, these receive their motor filaments from the *pharyngeal* branches of this nerve. These parts then continue to act, and propel the food down the passive *œsophagus*, as they would force it into any inorganic tube. That food should also be sometimes found in the *œsophagus* after death in some of those animals which had not eaten subsequent to the section of the nerves, is also exactly what we would expect. For it is evident, that if efforts at vomiting come on, the powerful contractions of the abdominal muscles will force the food into the paralyzed *œsophagus*. It could also easily be shown that the powerful contractions of the abdominal muscles necessary for coughing, would also force the food from the stomach into the *œsophagus*.

This arrestment of the movements of the muscular fibres of the *œsophagus* in deglutition after section of the *pneumogastrics*, when taken along with the fact observed in the two last experiments, proves, that this does not depend upon any diminution in the contractility of these muscular fibres, but upon a breach being made in the nervous circle, which, through the intervention of the *medulla oblongata*, connects the muscular with the mucous coat. We therefore conclude that the muscular contractions of the œsophagus are not caused by the ingesta acting directly as an excitant upon the muscular fibres through the mucous membrane, but by a reflex action, part of the *œsophageal* filaments of the *par vagum* acting as motor, and others, in the manner of sensitive nerves.[1] We believe that this fact affords a more satisfactory solution than has yet been offered of a physiological problem which has lately excited

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[1] Or, to use the phraseology of Dr. M. Hall, the contraction of the œsophagus in deglutition is an excito-motory action—the filaments of the œsophageal nerves distributed in the mucous coat being the excitor, and those in the muscular coats being the reflex or motor.

a good deal of attention, viz., whether any of the sympathetic actions of Whytt (the "excito-motory actions" of Dr. M. Hall) can take place without the intervention of a sensation. The food is propelled along the œsophagus without our consciousness and without our volition, and yet we have seen, that, before the presence of the ingesta in this tube can excite its muscular fibres to contract and propel their contents onwards, the same conditions of the nervous system are necessary, as for the production of those sympathetic or instinctive actions which are not excited by mental acts.[1] For, first, an impression must be made upon the nervous filaments of the *œsophageal* branches ramified in the mucous coat. Secondly, this impression must then be transmitted along these filaments to the central organs of the nervous system. Thirdly, some other change must be transmitted backwards from the central organs of the nervous system (in this case the medulla oblongata) along the motor filaments to the muscular coat, by which the stimulation and consequent contraction of the muscular fibres are produced.[2] We have here, then, a natural sympathetic action in the human body occurring without the intervention of a sensation, and simply because the presence of such a sensation is not necessary for the proper performance of this function. From this and other facts which have been adduced in discussions on this question, I think we are fully entitled to argue that sensation is not a necessary condition for the production of those sympathetic

[1] Among the sympathetic or instinctive actions which require the intervention of a mental act, or the excitation of a sensation, we may enumerate laughter, weeping, &c. These require the agency of the brain for their performance.

[2] Mr. W. B. Carpenter has given a very excellent tabular arrangement of the voluntary and instinctive actions, accompanied with some very perspicuous remarks, in No. 132 of the Edinburgh Medical and Surgical Journal.

actions with which it is so closely linked. Besides, it can be shown that these sensations have been connected with their attendant movements for an evident purpose; and if this purpose is one not necessarily instrumental in the production of the movements themselves as muscular movements, but to further the ends for which they were designed, we have already succeeded in obtaining a sufficient reason for the presence of these sensations, without being obliged to believe that they are actually concerned in the production of these movements. To illustrate my meaning by an example :—when the *bladder* and *rectum* are filled by their usual contents certain impressions are made upon the *spinal* sensitive nerves distributed in their mucous coat, and these, when conveyed to the central organs of the nervous system, excite certain sensations; upon this certain muscular movements follow by which the contents of these organs are expelled. Now the presence of the sensations is no absolute proof that their excitation is necessary for the performance of these muscular movements, for it must be at once obvious, that they here serve most important purposes, independent of any supposed influence exerted in the actual production of the movements. For if these muscular movements by which the contents of the rectum and bladder are expelled, were always to occur when the impressions were made, very serious inconveniences would evidently be occasioned. It is necessary, then, for our own comfort and well-being, that these movements should be influenced to a very considerable extent by volition; and of course this could only be accomplished by associating sensation with the excitation of the impression. When we take these circumstances into account along with the experiments we have stated, we may, I believe, safely conclude, that these sensations are not concerned in the production of these movements, as muscular movements, but have been superadded for an ulterior purpose.

The same kind of reasoning could easily be applied to all the other instinctive and sympathetic actions found conjoined with sensation in the healthy condition of the body, but which do not require the intervention of the brain for their performance.

The results obtained from these experiments upon the œsophageal branches of the *par vagum*, are at direct variance with the Hallerian doctrine, that the muscular fibres of the œsophagus are called into contraction by a direct impression. Though this does not occur in the healthy condition, it is probable that both the muscular fibres of the œsophagus, and also the principal closing muscles of the glottis, may be called into action by a direct impression, when the mucous surfaces of these organs are inflamed. For we find in many cases of disease or injury of the spinal cord, that the bladder ceases to expel its contents. In some of these cases, as I have myself seen, the urine after a time no longer requires to be drawn off by the catheter, but is constantly expelled as soon as it has accumulated in small quantities. On dissection of such cases after death, the mucous coat is found thickened and inflamed, and the muscular coat is greatly increased in strength and contracted upon itself. We believe that the explanation of such cases is this, that as long as the mucous coat is in a healthy condition, the urine fails to excite the action of the muscular coat by a direct impression, but when the former becomes inflamed this is effected.

The arrestment of the movements of the *œsophagus* in deglutition after section of the *par vagum* completely invalidates the evidence adduced by Brachet,[1] in favour of the dependence of the sense of satiety upon the integrity of the *par vagum*. We do not wish to deny that this may be the

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[1] Oper. cit., chapitre iii. § i. 1830.

case; we only mean to affirm that the data upon which Brachet rests his conclusions are obviously insufficient.

Cardiac Branches of Par Vagum.—It may be considered as perfectly ascertained by numerous observers, that section of the *par vagum* above the origin of the cardiac branch of this nerve does not materially affect the heart's action, and that the sudden death, occasionally remarked after the division of these nerves, and attributed by some of the early experimenters to arrestment of the action of this organ, is in fact dependent upon the suffocation of the animal, by suspending the movements of the muscles of the glottis. We may then, I believe, fairly attribute the increased frequency of the pulsations of the heart, observed during and for some minutes after the experiment, to the struggles and terror of the animal, for in a short time the action of the heart is as slow and vigorous as before the section of the nerves. Though the contractility of the heart is independent of the brain and spinal marrow, yet we know, from the experiments of Legallois, Wilson Philip, and others, that injuries of these organs influence the contractility of the heart in a most important manner. A sudden injury, extending to a considerable portion of the brain, arrests or at least much enfeebles the heart's action, and the extent to which the contractions of this organ are affected by mental emotions is well known. It is generally believed that it is through the cardiac branches of the *par vagum* that this influence is transmitted from the brain to the heart; but I am not aware that there are any decisive facts in proof of this opinion.

25*th Exper.*—Two rabbits were killed by crushing the brain extensively and suddenly by blows with a hammer. In one of these a portion of each of the pneumogastrics above the origin of the superior laryngeals had been removed. On exposing the heart of the rabbit, whose nerves had been left entire, which was done as expeditiously as

possible, these contractions of the heart were extremely rapid and very feeble. On exposing the heart of the animal whose nerves had been previously cut, which was also done immediately after the brain had been crushed, the action of the heart was evidently much slower and more vigorous than in the other animal. This comparative experiment was again repeated with the same results. The action of the heart was also examined in other two rabbits, whose pneumogastrics had also been previously divided in a similar manner before the brain was crushed, and in neither of these did it present the same rapid and feeble movements, which I have always witnessed when these nerves were entire.[1] Now since the contractility of the heart can also be effected by extensive and sudden injury of the spinal cord after the brain has been removed, it would appear that the influence of causes acting on the central organs of the nervous system may be transmitted to the heart by two channels, viz. by the *par vagum*, and by the *sympathetic system*. In these experiments, the trunk of the sympathetic in the neck was divided along with the *par vagum*, but this of course could not affect the greater number of the sympathetic nerves passing to the heart.

Pulmonary Branches of the Par Vagum.—The only method of experimenting on the pulmonary branches of the *par vagum* that can be adopted in the living animal, is that of cutting the trunk of the nerve itself, as it lies in the neck.

Effects of Section of one Pneumogastric Nerve upon the Lung of that side.—About two years ago I commenced, at the suggestion of Dr. Alison, a series of experiments upon the effects of section of the *pneumogastric* nerves upon the *respiratory murmur*, as it might probably enable us to

[1] Dr. Alison was present at one of these experiments, and made the same remark.

ascertain what is the cause of the first departure from the healthy condition of the lungs after section of these nerves. As I was led by the statements of Wilson Philip[1] to believe that section of one of the *pneumogastrics* would produce the usual morbid changes in the lungs which are observed after the section of both, and by the results of the experiments of Magendie[2] and Mr. Swan,[3] to expect that these would be confined to the side operated upon, I selected this method of experimenting, as the sound lung in that case would afford an accurate standard of comparison, by which to judge of the changes effected. No doubt the well-known experiment of Haighton[4] is inconsistent with the supposition, that section of one of the nerves destroys the function of the lung on the same side; but as he simply divided the nerve without removing a portion of it, the re-union of the cut ends might have occurred before a sufficient time had elapsed to permit these morbid changes to be effected. I have removed a portion of the *par vagum* in fourteen animals, and have never yet observed any morbid structural change that could be attributed to the section of the nerve. Two of these were calves, and were killed about twenty-four hours after the section of the nerve: seven of them were dogs, and were allowed to live from four to ten days: three rabbits were allowed to live a fortnight: one rabbit lived three months, and one rabbit was killed after six months. None of these animals appeared to suffer any bad consequences from the operation. The dogs breathed easily, and ate and digested as before; the rabbits also were as lively and active as ever, after the operation. No morbid changes, as I have said, could be observed in the

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[1] Oper. cit., p. 145. [2] Oper cit., p. 401.

[3] Essay on the connexion of the Heart and Arteries, and the Functions of the Nervous System, &c. 1829.

[4] Medical Facts and Observations, vol. vii. p. 163, Experiment IV.

lungs of the calves and dogs after death. The three rabbits which were allowed to live for a fortnight were apparently in perfect health when killed, and the lungs were shown to my friend Dr. Knox, who was at the same time requested to examine if there was any structural difference between the lungs of the opposite sides, but he could perceive none. The rabbit which lived three months died of recent pneumonia, affecting *both* lungs equally. The lungs of the rabbit killed at the end of six months were perfectly healthy. In all these experiments, a sufficiently large portion of the nerves was removed to prevent their re-union. The morbid changes on the lung in the side on which the nerve had been cut, observed by Magendie and Swan, must have been accidental. The experiments which I have mentioned are sufficiently numerous to entitle us to conclude, that lesion of one of the pneumogastrics does not necessarily, or even generally induce disease of the lung of that side. This does not appear so remarkable, when we remember the free anastomoses between the pulmonary plexuses of the two sides.

Effects of Section of the Par Vagum upon the respiratory muscular movements.—As the pulmonary branches of the pneumogastrics are the only nervous filaments the lungs receive from the cerebro-spinal axis, we would expect, in accordance with the generally received notions on the physiology of the nervous system, that all the impressions made on the mucous surface of the lungs which excite the sensations, and sympathetic movements connected with the respiratory functions, are conveyed to the cerebro-spinal axis through this nerve. And this opinion is strengthened by the experiments of Legallois and Flourens, by which it was shown, that though all the other parts of the encephalon above that portion of the *medulla oblongata* where the *par vagum* is attached, could be removed without arresting the respiratory muscular movements, yet these instantly

ceased, when this part of the nervous system was injured. And it is further well known, that if the spinal marrow be cut across, all the respiratory muscles are paralyzed which receive their nerves from that portion of the spinal cord below the point divided, while those which receive their nerves from that portion of the spinal cord that remains in connexion with the *medulla oblongata*, continue to perform their usual functions. From these and other facts, it may be considered as ascertained, that all the impressions made at the lungs must be conveyed to the *medulla oblongata*, before they can excite sensations or sympathetic movements. That the *par vagum* can carry these impressions from the lungs, is not only rendered probable by the connexion of the *par vagum* with the *medulla oblongata*, but may be considered as proved by the results of the experiments upon section of the spinal cord, to which we have just referred. It, however, by no means necessarily follows, that this is the sole channel by which these impressions are conveyed to the *medulla oblongata*, for it is possible that these might reach the spinal cord through the branches of the sympathetic distributed upon the lungs, and in this way be conveyed upwards to the *medulla oblongata*. This last supposition is certainly not a very probable one, and can only be admitted after satisfactory evidence in its favour.

We shall now examine how the facts accord with the theories on this question. That the respiratory movements continue after section of both pneumogastrics is now universally admitted, but physiologists are by no means agreed in what manner these proceed. Those who believe that the movements of the respiratory muscles are usually carried on by the exercise of volition, will of course maintain, that these proceed exactly in the same manner as before the section of the nerves. This being, however, an untenable supposition, we shall not discuss it further. Those, on the other hand, who maintain that this function, though

capable of being influenced and controlled to a very considerable extent by volition, is nevertheless generally performed without our volition, and in consequence of impressions made at the lungs and conveyed to the *medulla oblongata*, must of course believe that this function is carried on differently after section of these nerves, from what it is in the natural condition of the body. Brachet asserts that an animal continues to breathe after section of the pneumogastrics, because it has acquired the habit of using the respiratory muscles.[1] Dr. Marshall Hall believes that when these nerves are divided, the function of respiration is no longer an involuntary and a sympathetic movement, or, as he expresses it, *excito-motory*, but is now a cerebral function, or, in other words, is performed by an act of volition.[2] That these movements continue after section of the *pneumogastrics*, and under circumstances where it is impossible that they can be carried on by an act of volition, I have been reluctantly compelled to believe. For I am satisfied that these respiratory movements will not only go on regularly and vigorously for hours together in kittens one and two days old, but that they will also proceed in animals deprived of all volition by a small dose of prussic acid.[3] Thinking that it might be possible, though certainly not probable, that the impressions made at the lungs may in these cases reach the *medulla oblongata* through the free anastomoses between the laryngeal nerves at the larynx, (for in the first experiments I cut the nerves, as is gener-

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[1] Oper. cit., p. 132, "à l'habitude que le système nerveux cerébro-spinal a contractée de faire mouvoir les muscles respirateurs." How an animal could contract the habit of performing an *involuntary* action, it would certainly be difficult to explain.

[2] Philosophical Mag., January 1835, pp. 71, 72, and Lectures on the Nervous System, p. 25. 1836.

[3] In one dog in which these nerves were divided, the respiration continued above twenty minutes after we had satisfactory evidence that the sensorial functions were arrested.

ally done, in the middle of the neck,) I proceeded to repeat the experiments by dividing the nerves near the base of the cranium, and above the origin of the superior laryngeals. The results, however, were the same as when the nerves were cut in the usual manner.

We are next naturally led to inquire, how the *sensation arising from the want of fresh air in the lungs*, or the *besoin de respirer*, is affected by section of these nerves? Brachet[1] has related some experiments, which appear to prove in a very conclusive manner, that this *besoin de respirer* is annihilated by section of the pneumogastrics. Though I considered the details given by Brachet as not satisfactory on some points, yet, after reading them, I had no doubt of the accuracy of his opinion. I am now, however, perfectly satisfied, from numerous experiments, that an animal will continue to evince great uneasiness after section of these nerves, when the access of air to the lungs is prevented. I have repeated these experiments in several ways, and with the same results. One of the methods followed was this:—

26*th Exper.*—The *par vagum* was cut on both sides in two kittens ten days old. When one of these nerves was cut, the blood in the carotids became dark-coloured, but it soon after resumed its natural appearance. When both nerves were cut, the blood in the carotids again became dark, the animal began to struggle, and it was necessary to open the trachea to prevent their immediate suffocation. After the trachea was freely opened, the blood in the carotids again assumed its arterial colour. Both animals struggled, made violent inspirations, and appeared to suffer great uneasiness when the access of air to the lungs was prevented. On compressing, for example, the exposed trachea with the forceps, the animals lay quiet for a few moments, making several inspiratory efforts gradually increasing in force and

[1] Oper. cit., pp. 133-5.

extent; after a few moments more, the inspirations became heaving, and they began to struggle and appear very uneasy.

This experiment was repeated with similar results upon a kitten two days old. The nerves in this last experiment were cut above the origin of the superior laryngeals. I have also performed a similar experiment upon several rabbits and dogs. In all of these animals the breathing became very laboured, and the animal evinced every indication of great uneasiness, when the access of air to the lungs was *suddenly* and *fully* prevented.

From these experiments I conclude that section of the *par vagum* on both sides, neither arrests the transmission of those impressions made upon the mucous surface of the lungs, by which the respiratory movements are excited; nor does it annihilate the sense of anxiety arising from the want of fresh air in the lungs. We are obliged, then, to suppose, that these impressions are conveyed backwards by the sympathetic branches distributed upon the lungs to the spinal cord, and thus pass upwards to the *medulla oblongata*. As has been already stated, we believe that these impressions may also be transmitted through the *par vagum*. It is quite possible that the latter is the usual channel by which these are transmitted, and it is only when they become more intense that they pass along the other course we have mentioned. This may, perhaps, explain the slow respiration occasionally observed after section of the pneumogastrics. It is certainly contrary to the usually received physiological doctrines to suppose that the sympathetic system can, in the *healthy* state of the nerve and of the parts upon which it is distributed, transmit those impressions by which the natural sensations and sympathetic movements are excited, but I cannot see how the facts can otherwise be explained. The peculiar pain felt on bruising the healthy testicle, and which we are led to believe depends

upon the sympathetic, may also induce us to believe that a previously inflamed condition of the filaments of this nerve is not absolutely necessary to enable it to transmit those impressions which excite sensation.

Brachet[1] has also detailed several experiments which seem satisfactorily to prove that all the sensations occasioned by the presence of foreign bodies, &c., in the air passages are dependent upon the integrity of the pneumogastrics. I have repeated these experiments on several dogs and cats, but have not yet been able to arrive at any conclusive results. The chief difficulty I have experienced in making any satisfactory experiments on this question has arisen from the great insensibility of the mucous membrane of the lungs in the healthy state. I have never yet been able to induce the severe paroxysms of coughing described by Brachet, by any mode of irritating the inner surface of the trachea and bronchi which I have adopted, if care be taken to prevent the application of these irritants to the inner surface of the larynx, when the superior laryngeals are entire. I injected by a syringe two drachms of alcohol slightly diluted, down the trachea of one dog, and as much *eau de cologne* into that of another, when both nerves were entire, without exciting any decided cough. The friction of a probe against the inner surface of the trachea and bronchi never produced cough. Though I have seen some reason to doubt that these sensations of the mucous surface of the lungs are affected to the extent stated by Brachet, yet I do not at present wish to offer an opinion upon this subject, until I have investigated it more thoroughly. I would only suggest at present, that it is possible that Brachet in some of his experiments may have overlooked a source of fallacy which would very seriously interfere with the results—and that is, the facility with which some of the irritants used

[1] Oper. cit., pp. 157-9.

might reach the interior of the larynx; for it is to be remembered, that the nerves were cut below the origin of the superior laryngeals. If the head be depressed when a quantity of fluid is thrown into the trachea, or if the fumes from a muriatic acid bottle are used as an irritant, (as by Brachet in some of his experiments,) the chances are, that a part of these will reach the interior of the larynx, and excite violent efforts to cough. Perhaps this may explain in some measure the discrepancy between the results obtained by Brachet upon the sensibility of the mucous membrane of the trachea, and those obtained by practical surgeons in operations on the human species, and in some experiments by Haller[1] upon animals. Some of the animals I experimented on did cough slightly when fluids were injected into the trachea before the nerves were cut—at least in the imperfect manner in which an animal must do when the trachea is opened.

Morbid changes in the Lungs after section of the Pneumogastrics.—Section of the pneumogastrics invariably proves fatal if the cut ends of the nerves are kept apart. The animal seldom if ever lives beyond three days, and generally dies sooner; but when the cut ends of the nerves are allowed to remain in contact, it sometimes lives ten or twelve days. There can be now no doubt that the section of these nerves proves fatal by its effects upon the lungs. The congested state of the blood-vessels of the lungs, and the effusion of frothy serum into the air-cells and bronchial tubes, may be considered as the characteristic and only constant appearance after death from section of the pneumogastrics. I am of course here supposing that the air is allowed to pass freely into the lungs. It is quite evident that section of these nerves does not prove fatal by removing any innervation

[1] Opera Minora, tom. i., p. 402. Laus. 1762; or Sur la Nature Sensible et Irritable, tom i., p. 394. Laus. 1756.

necessary for effecting the changes by which the venous blood is converted into arterial, as Dupuytren supposed; nor by coagulating the blood of the pulmonary arteries, as Mayer maintains. The first point to ascertain in an investigation of this kind, is the first departure from the healthy condition of the organ—to decide whether the effusion of the frothy reddish serum, by interfering with the usual changes of the blood in the lungs, causes the congested state of the pulmonary blood-vessels, and the severe dyspnœa, as is usually imagined; or whether this effusion is the effect of a previously congested state of the pulmonary blood-vessels and its attendant *dyspnœa.* If it be made out that the fatal dyspnœa is not the effect of the effusion of serum, we have next to inquire what is the probable cause of this congested state of the pulmonary blood-vessels and the accompanying dyspnœa. I have been collecting for some time past a considerable number of facts on these questions, but have not yet been able to obtain what I consider satisfactory data. Upon the discussion of this subject I shall not at present enter, but reserve it for another opportunity. If I hazard a few remarks upon this question, I wish that they may be looked upon as conclusions in which I myself do not place any very great confidence, until I have been more thoroughly satisfied of their accuracy. I am at present inclined to believe, from the experiments which I have made, that this frothy serous effusion is the *result* of the congested state of the pulmonary blood-vessels attending the severe dyspnœa which precedes death, and not the *cause* of this *dyspnœa.* The grounds on which I have adopted this opinion are these:—1*st,* This frothy serous effusion is exactly similar to the fluid in the air-passages in death from whatever cause, when preceded by protracted and severe dyspnœa. 2*d,* The extent of this effusion appears to be proportionate to the duration and severity of the dyspnœa preceding death. 3*d,* Dyspnœa is sometimes

present after section of these nerves when the passage of air into the lungs is quite free, and before any serous fluid has been effused. What is the cause of the dyspnœa and congestion of the pulmonary vessels, I have not yet been able to satisfy myself, but I believe it probably depends upon paralysis of the muscular fibres of the bronchi. If these muscular fibres move in unison with the muscles of respiration, like those of the *larynx* supplied by the same nerve, they must exert a very important influence over the renewal of the air in the lungs. That we are not conscious of any such movements cannot be urged as an objection to this view, after what we have seen in the *œsophagus*. Of the existence of the muscular fibres of the *bronchi* described by Reisseissen there can be no doubt. I have seen these muscular fibres very distinctly in some of the smaller bronchial tubes. I have observed a circumstance in experiments upon dogs, which, if confirmed by more extended investigation, may throw some light upon this question. In four dogs in which the *par vagum* was cut on one side, the respiration of the same side, though quite natural immediately after the operation, became distinctly bronchial after a few hours. This continued for two or three days, and the natural respiratory murmur gradually returned. One of these dogs was killed during the continuance of this bronchial respiration, and no structural change could be detected in the lung. As I have, however, some reason to doubt from late experiments whether this is a constant occurrence, I shall defer its consideration until I have made more extended observations. I am inclined to believe from what I have seen, that pneumonia is a pretty frequent occurrence after section of these nerves.

Gastric Branches of the Par Vagum.—As my experiments on these branches are not yet completed, I shall reserve this part of the investigation for a future opportunity.

Part Third.—*Spinal Accessory Nerve.*

Before stating the experiments I have made upon this nerve, I wish to advert for a little to its origin and distribution, and in this I shall restrict myself to those points which are most intimately connected with the discussions upon its functions. The origin of the spinal accessory from the spinal cord is sometimes higher and sometimes lower, but we have the authority of Huber,[1] Lobstein,[2] and Bellingeri,[3] who have attended particularly to this subject, in stating that it most frequently commences opposite the *sixth* or *seventh cervical nerves*. This nerve, in its course upwards to the *foramen magnum*, is placed between the posterior roots of the spinal nerves and the *ligamentum denticulatum*, and receives its filaments, as Bellingeri[4] has clearly demonstrated, from the lateral or middle column of the spinal cord. The filaments of the *spinal accessory* may come entirely from the *middle column* of the *spinal cord*, or it may also receive some filaments from the posterior roots of the first and second *cervical nerves*. These filaments, from the posterior roots to the *spinal accessory*, when present, rarely come from the second, but generally from the first cervical. When they come from the posterior root of the second cervical, they are few in number. Those from the first cervical vary considerably in number; for we find sometimes a few, sometimes the greater part,

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[1] Huber says at p. 13, "De Medulla Spinali et Speciatim de Nervis ab ea Provenientibus," that it commences opposite the seventh cervical, but he afterwards places it opposite the sixth.

[2] Lobstein (de Nervo Spinali ad Par Vagum Accessorio, p. 341, tom i. Thesaus. Diss. Sandifort, 1768,) says it commences under the sixth pair of cervical nerves by a slender beginning.

[3] Bellingeri (De Medulla Spinali, Nervisque ex ea Prodeuntibus, p. 74, 1823,) adopts the description of Huber where he states that it commences opposite the seventh.

[4] Oper. cit., pp. 51 and 55. See also Edin. Med. and Surgical Journal, vol. xlii., p. 396. 1834.

at other times the whole of the filaments of this root passing to join the *spinal accessory*. This junction between these two nerves may be confined to one side of the spinal cord, or it may be present on both. This communication between the *posterior root* of the first cervical and the *spinal accessory* is far from being rare, and Lobstein asserts that it is more frequently present than absent.[1] When the posterior root of the first cervical joins itself to the *spinal accessory*, a branch of equal size leaves the trunk of the *accessory*, either at the point where it is joined by the posterior root, as figured and described by Asch,[2] or from three to six lines above this junction, as figured by Huber,[3] and described by Bellingeri.[4] This branch, after leaving the accessory, proceeds outwards, approaches the anterior root of the first cervical, and takes the place of the posterior root. When the posterior root of the first cervical comes from the *accessory*, it generally swells into a ganglion in the usual position. Sometimes, however, though rarely, a ganglion is found where the posterior root leaves the *accessory* to join itself to the anterior root. This ganglion was first pointed out by Huber, and its existence has been denied by Lobstein, Asch, Haller, and Scarpa, and has again been described by Bellingeri. I have seen this ganglion once, and it was present on one side only. It becomes an interesting question, to know whether or not the whole of the filaments joining the *accessory* from the posterior roots of the *spinal nerves*, leave it again to form the posterior root of the *first cervical*. Bellingeri answers this question in the affirmative.—" The filaments coming

[1] In speaking of the filaments which form this communication, he says, "communicationem illud notamus quod sæpius accessorium subire quam eundem intactum relinquere observentur."

[2] De Primo Pare Nervorum Medullæ Spinalis. Tab. x. fig. ii. et Explicatio, p. 335. Ludwig, Sc. Nerv. Min. Sel. tom. i.

[3] Oper. cit.

[4] Ibid., p. 80.

from the posterior roots to the accessory are not intermixed, but only approximated, so that they can be separated by slight traction."[1] And in another place he says, "I believe that the filaments from the posterior roots which join the accessory, leave it again to proceed to the posterior root of the first cervical."[2] From this he concludes that this nerve contains no sensitive filaments. Müller[3] adduces some unusual appearances in this nerve, observed by Hyrtl, Remak, and himself, which would seem to favour the opinion, that it contains some sensitive filaments, independent of those it occasionally receives from the posterior roots. "I do not, however, affirm," he says, "that the spinal accessory always contains a sensorial element, but leave it doubtful."—"But in the case," he continues, "where the *nervus accessorius* forms an intimate connexion with the posterior root of the first cervical or any other nerve, we may suppose an interchange, and this in the same degree will render probable the idea of Monro, that the communication of the spinal accessory with the posterior root of the first or any other spinal nerve will be an equivalent to it for a posterior root."[4] The only other point connected with the anatomy of this nerve to which we shall here refer, is its connexion with the *par vagum;* for we shall have occasion to allude to it repeatedly, when examining the functions of this nerve. As the spinal accessory is passing through the *foramen lacerum* it divides into two branches—an internal and an external. The internal, after giving off some filaments to assist in forming the pharyngeal branch of the *par vagum*, becomes incorporated with the filaments of the trunk of the *nervus vagus*. The *nervus vagus* swells into a ganglion, (*ganglion secundum nervi vagi,*) where it is joined

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[1] Oper. cit., p. 81. [2] Ibid., p. 79.
[3] Arch. für Anat und Physiol., 1837, S. 279.
[4] Oper. cit., p. 279.

by the internal branch of the accessory, and by branches from the sympathetic. This ganglion is very distinct in the lower animals. Bischoff[1] states, that he ascertained by dissections of the Mammalia that only a part of the fibres pass into the ganglion, and that the others retain their fibrous character. He also adds, that it is evident that those which form the ganglion belong to the *par vagum*, while those which do not enter into its formation belong to the internal branch of the accessory. From this gangliform swelling, the superior laryngeal nerve arises.[2] The *external branch* proceeds outwards, perforates the upper part of the *sterno-cleido-mastoideus*, sends filaments to this muscle, and to the *trapezius*, and forms at the same time

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[1] Oper. cit., p. 22.

[2] Since the above remarks were written out, I have seen it announced in a paper by Mr. E. Cock, in Guy's Hospital Reports for October 1837, that Sir Astley Cooper has discovered a new ganglion in the rabbit, which he terms the superior laryngeal ganglion. This, however, is obviously the *second ganglion* of the par vagum of some anatomists. It appears that Sir A. supposed, before he discovered this ganglion, that the superior laryngeal nerve furnished an objection to the opinion of Sir C. Bell, that all nerves of common sensation are provided with ganglia. But Sir A. seems to have forgotten that there is a true ganglion of the par vagum as it lies in the foramen lacerum. This superior ganglion of the vagus was known to Ehrenritter, figured by Wutzer, (De Corporis Humani Gangliorum, Fab. et Usu, Fig. vii., 1818,) redescribed by Arnold, (Der Kopftheil des vegetativen Nervensystems, &c., p. 105, Heid. 1831;) also particularly described by Bischoff, (Nervi Accessorii Willisii, Anat. et Physiol. p. 21, 1832;) and now admitted by every anatomist who has examined it. The second ganglion of the par vagum, which Mr. Cock thinks an entirely new discovery on the part of Sir Astley, has also long been known. It has been described by Vieussens, Willis, Scarpa, and others. It is figured by Wutzer (Oper. cit.) as the second ganglion of the par vagum. Bischoff, after stating that Arnold names this swelling upon the par vagum a gangliform plexus, thus expresses himself: "Verum apud mammalia dubito an argui possit hunc tumorem non reapse ganglion esse. In universum proportione in his bestiis multo crassius est quam in humano corpore et simillimum sæpe est ganglio cervicali supremo Sympathetici quod situm est aut supra aut juxta aut infra illud vagi ganglion." (Oper. cit., p. 22.) In Tab. ii, he gives some very excellent engravings, in which the position and form of this second ganglion of the par vagum, and the origin of the superior laryngeal from it are represented in the cat, fox, sow, mole, and weasel.

several anastomoses with branches from the *cervical plexus.* The peculiar origin and course of this nerve, and particularly its intimate connexion with the *par vagum,* have formed the basis of most of the speculations on its functions since the time of Willis. It was maintained by Willis,[1] that this nerve, from its connexion with the *par vagum,* regulated those involuntary movements of the neck and arm connected with the emotions and passions. Lobstein likewise believed that the spinal accessory joins the *nervus vagus* for the purpose of connecting itself with the involuntary functions,[2] and he supposed that paralysis of this nerve might also affect the movements of the *pharynx* and *larynx.*[3] Others have maintained that it is a nerve of involuntary motion, from the particular portion of the spinal cord from which it arises. It is, as is well known, one of Sir C. Bell's respiratory nerves, arising, as he supposes, from a particular tract in the spinal cord. According, then, to Sir C. Bell, it is a nerve of involuntary motion. Bellingeri believes that the lateral tract of the spinal cord from which the accessory arises, presides over the instinctive and sympathetic movements, and consequently this nerve must be one of involuntary motion.[4] Arnold,[5] Scarpa,[6] and Bischoff,[7] maintain that the accessory stands in the same relation to the *nervus vagus* which the anterior roots of the spinal nerves do to the posterior.[8] These nerves, as they lie in the *foramen lacerum,* do certainly resemble very closely in appearance the anterior and posterior roots of the spinal nerves. The junction of the

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[1] Cerebri Anatome, &c., Cap. xxviii. [2] Oper. cit., p. 346.
[3] Oper. cit., p. 345. [4] Oper. cit., pp. 89, 90.
[5] Der Kopftheil des vegetativen Nervensystems. Heidelberg, 1831.
[6] De Gangliis Nerv. deque Essentia Nervi Intercost. Ann. Univers. de Medicina. 1831. [7] Oper. cit.
[8] It appears that this idea had been previously stated by Görres, (Exposition der Physiologie. Coblentz, 1805,) as quoted by Müller.

internal branch of the accessory with the *par vagum* beyond the part where it swells into its superior ganglion increases the resemblance still farther. To this opinion we shall have again occasion to refer. With these preliminary remarks, I shall now proceed to give the results obtained in my experiments on this nerve.

Effects of Irritating this Nerve in the Living and recently killed Animal.—These experiments on the living animal were performed on the external branch of the *accessory* before it perforates the upper part of the *sterno-mastoid*, as it is impossible to operate on the trunk of the nerve unless it is exposed within the cranium. Irritation of this nerve by the forceps in the living animal produces vigorous convulsive movements of the *sterno-mastoid* and *trapezius*, as was remarked by Dr. M. Hall and Mr. Broughton in their experiments.[1] This mode of irritating the nerve is not attended by any indications of suffering, unless the nerve is strongly compressed between the blades of the forceps. When the nerve is firmly included in a ligature, the animal gives very decided evidence of suffering pain. When the nerve has been firmly tied or cut across, irritation of the lower end is attended by the same convulsive movements of the muscles, while irritation of the upper end, or that in connexion with the *spinal cord*, is unattended by any muscular movements. The same muscular movements are observed on irritating the nerve in the recently killed as in the living animal. From these experiments we conclude that the filaments of the *external* branch of the *spinal accessory* are principally motor, and that the sensitive filaments must be very few in number. Whether those sensitive filaments belong originally to the nerve, or whether it obtains them from the other nerves at the base of the

[1] Fourth Report of British S. Association.

cranium, with which it anastomoses, we cannot at present determine.

Effects of the Division of these Nerves.—According to Sir C. Bell, the section of the *spinal accessory* paralyzes the muscles to which it is distributed, as muscles of respiration, though they still retain their voluntary movements through the media of the spinal nerves. This appears to be established by the following experiment performed by Mr. Shaw:—" The *par vagum* of an ass was first divided, with the intention of causing difficult and laboured respiration. When all the respiratory apparatus was in great agitation, and when the sterno-maxillaris (the same as the sterno-cleido-mastoideus in man) was especially in action, the spinal accessory was divided. In an instant the sterno-maxillaris ceased to act as a muscle of respiration, but when the animal struggled to get free, it became rigid, showing that through the plentiful supply of spinal nerves it still retained its office of moving the head and neck."[1] That part of the experiment which relates to the cessation of the respiratory movements of the *sterno-maxillaris* being a negative observation, ought, we think, to have been repeated and further confirmed, before such important conclusions were drawn from it. I have removed a portion of the external branch of this nerve on one side as it issues from the foramen lacerum in seven dogs, without observing any change upon the ordinary voluntary movements of that side of the neck. It is possible, however, that if the animals had any violent voluntary efforts to make with the muscles of the neck, that those of the side on which the nerve had been cut might have acted less vigorously than those of the opposite side. I then proceeded to endeavour to ascertain the effects of the section of this nerve upon the

[1] London Medical and Physical Journal, vol. xlix., p. 459. 1823.

involuntary respiratory movements of these muscles. The experiments were performed on dogs and cats.

The plan which I followed appears to me to be more likely to lead to accurate results than that adopted by Mr. Shaw. In the method followed by Mr. Shaw the animal might use these muscles voluntarily when the breathing was rendered difficult, and therefore lead to erroneous conclusions. Besides, the difficulty of recognising the movements of any particular muscle through the skin of the animal, (at least in the kind of animals I operated on,) when so many other of the neighbouring muscles are in movement, must also be apt to mislead. The plan which I adopted was this:—A small dose of prussic acid was given to an animal on which the spinal accessory had been previously divided on one side. After the convulsive movements produced by the effects of the acid had ceased, the animal was generally found in a state similar to what we sometimes see in apoplexy—the action of the heart went on, and the respiration was slow, heaving, and performed at considerable intervals, while the sensorial functions appeared to be completely suspended. The animal had undoubtedly lost the power of making the slightest voluntary effort. The respiratory movements always ceased before the action of the heart; but they continued in several of these animals sufficiently long to permit us to lay bare all the muscles of the anterior part of the neck, and to make accurate observations. The *sterno-mastoids* in dogs and cats appear to have little action as respiratory muscles; for in some of these experiments, though the *sterno-hyoid* and *thyroid muscles* acted very powerfully in unison with the other muscles of inspiration, and the head was pulled towards the chest at each inspiratory movement, yet no contraction could be perceived in the *sterno-mastoid*, neither in the side on which the nerve had been cut, nor on that on which it had been left uninjured. In one dog, distinct

muscular movements were observed in a bundle of muscular fibres, which arise from the humerus, run up along the outer margin of the *sterno-mastoid*, and are inserted along with it. This muscular bundle receives filaments from the *accessory*. In two dogs and one cat in which the head was fixed, and where these respiratory movements were particularly vigorous, distinct movements of contraction and relaxation were observed in the exposed *sterno-mastoid* muscles, synchronous with the other muscles of respiration. These were, perhaps, somewhat weaker on the side on which the nerve had been cut. In one of these dogs similar movements were observed in the *trapezius* on the side on which the nerve had been divided. From these experiments we conclude, that the *sterno-cleido-mastoideus* and *trapezius* can assist in the involuntary movements of respiration after section of the *spinal accessory*, and therefore it cannot be called the special respiratory nerve of these muscles. As far as we can observe, the functions of the external branch of the *spinal accessory* exactly resemble those of the filaments coming from the *cervical plexus*, with which it anastomoses so freely. Future anatomical researches may perhaps explain to us how it follows this peculiar course, without obliging us to suppose that it has a reference to any special function in the adult of the human species.

We now proceed to inquire what are the functions of the *internal branch* of the spinal accessory? We have already stated that Arnold, Scarpa, and Bischoff have imagined that the spinal accessory stands in the same relation to the *par vagum* as the anterior roots of the spinal nerves do to the posterior. If this view be correct, the *internal branch* of the *accessory* must furnish the motor filaments of the *par vagum*, and upon it must depend the important muscular movements of the pharynx, larynx, and œsophagus. As no anatomist has yet succeeded in tracing for any great distance

the filaments of the accessory, separate from those of the trunk of the *nervus vagus*, it is therefore impossible to decide this question satisfactorily by the anatomy alone of the nerves.[1] Bischoff proceeded to submit this opinion to the test of experiment, by endeavouring to ascertain what effects section of the trunk of the accessory within the cranium would have upon the voice. After relating several failures upon dogs, in which the animals either died from hemorrhage before the nerves could be fairly divided, or when they did survive the operation, it was afterwards found that the superior filaments of the nerve were left uncut; he informs us that he at last succeeded in performing the experiment in a satisfactory manner upon a goat. He states that it was remarked in this experiment, that as the roots of the accessory nerves were cut, the voice became gradually weaker, and when the last filaments were divided, it became completely lost.[2] It must, however, be apparent, that one negative experiment of this kind can never be adduced as satisfactory evidence of the dependence of the movements of the *larynx* upon the *spinal accessory*. There were here other causes in operation which might suspend the power of emitting sounds besides the section of the roots of the *accessory* nerves, and the simultaneous occurrence of the

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[1] Since this was written I have seen in No. iii. of Müller's Archives for this year (1837) an epitome of the investigations of Bendz, "De connexu inter nervum vagum et accessorium." From these dissections, it is concluded (Jahresbericht, &c., im Jahre 1836, p. xxv.) that both in man and animals, the *nervus pharyngeus* of the *par vagum* is derived in a great part from the filaments of the accessory, and that the *nervi laryngi, superior* and *inferior*, and the *œsophageal plexus* also receive filaments from this nerve. He has also observed the accessory in man, (p. xxiv.) furnish a few filaments to the second ganglion of the *par vagum*. Into this second ganglion of the *par vagum* all the filaments of the vagus do not enter. He has even seen in the rabbit some of the filaments of the vagus pass the superior ganglion placed on this nerve, without entering into its formation.

[2] Oper. cit., p. 94.

complete loss of voice, and the section of the last roots of the *accessory*, might not have stood to each other in the relation of cause and effect, but as a mere coincidence. The escape of the cerebro-spinal fluid, the unavoidable loss of blood, the exposure of the *medulla oblongata* to the external air, and the protracted pain and struggles of the animal, are more than enough to induce a state of stupor and debility sufficient to suspend its cries. In two attempts to repeat this experiment upon a dog and a cat, the animals died from hemorrhage before the nerves could be fairly exposed and divided. I then determined to try the effects of irritating these nerves within the cranium on an animal recently killed. I find that this method had been previously followed in one experiment by Müller.[1] In this experiment he ascertained that irritation of the *nervus vagus* within the cranium, both mechanically and by a single pair of galvanic plates, produced contraction of the *œsophagus*. As I was not aware that such an experiment had been made until I had completed those which I am about to mention, I could not be influenced in my observations by the authority of this very distinguished physiologist and accurate observer. I performed my experiments in the following manner:—The animal was deprived of sensation by a dose of prussic acid, and the cranium was then opened as rapidly as possible. As soon as the nerves were exposed, the parts in the neck, which it was wished to observe during the irritation of the nerves, were quickly laid bare. The nerves were insulated from each other, and the animal was so placed, that while Dr. Duncan and Mr. Spence irritated the nerves, I could watch the effects upon the parts exposed. Some of the experiments failed from the facility with which

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[1] Handbuch, &c., Erster Band, p. 641.

these nerves are broken within the cranium, and from the enfeebled state of the muscular contractility, from the length of time required to go through all these preparatory steps. We procured, however, some results which I consider sufficiently satisfactory as far as they go. In one experiment, in which the *spinal accessory* was irritated by galvanism, when the aperture of the *glottis* was exposed, powerful convulsive movements of the shoulder were observed, but not the slightest movements of the muscles of the *glottis*. In another dog convulsive movements of the pharynx accompanied the convulsive twitches of the shoulder. In one dog in which irritation of the *spinal accessory* had produced powerful movements of the shoulder, the nerve was broken within the *foramen lacerum*, on attempting the irritation after the *glottis* was brought into view; the galvanic wires were then applied to the par vagum, and a distinct though feeble movement of the *arytenoid* cartilages followed, *unattended by any movements of the shoulder*. In another dog, a distinct movement of the *pharynx* and *arytenoid* cartilages followed the pinching of the insulated *par vagum* with the forceps. I attach most importance to this last experiment, as it is difficult in some cases to be quite certain that the galvanic influence is confined to the nerve operated upon, when others are placed in the immediate neighbourhood. From those experiments we think there can be no doubt that the trunk of the *par vagum* contains within it *motor* filaments independent of those which it receives from the *internal* branch of the spinal accessory. That the internal branch of the spinal accessory assists in moving the muscles of the pharynx we are satisfied, not only from the experiments just stated, but also from those upon the pharyngeal branch of the *par vagum*. Of the probable destination and functions of the other filaments of the *internal* branch of the *accessory*, we cannot pretend to judge without more extended inquiries. We certainly do not consider that

these experiments entitle us to assert that they are not motor filaments.[1]

I may here state, that the arguments adduced from comparative anatomy, in favour of the opinion, that the spinal accessory is solely connected with the involuntary respiratory movements, may be considered as completely controverted by the late researches. Serres has discovered its existence in some birds; Weber in some fishes; and Bischoff in birds, fishes, and reptiles.

These experiments upon the glosso-pharyngeal and spinal accessory have furnished results in direct opposition to the respiratory system of Sir C. Bell; for certain supposed functions of these nerves have been considered as forming one of the principal strongholds of that ingenious and plausible theory. I think there can be no doubt, that the distinguished discoverer of the distinct functions of the anterior and posterior roots of the spinal nerves, has taken a very limited and partial view of the sympathetic movements of the body, when he framed his respiratory system. His attention appears to have been so entirely absorbed by those connected with the respiratory function, that he seems to have forgotten that there are other extensive associated and sympathetic movements of the muscles of the body, besides those which he has so beautifully illustrated. For it is obvious, that, if a particular tract of the spinal cord is necessary to carry on the respiratory movements, there ought also to have been a defecatory tract, a urinary tract, and so on, to carry on the other sympathetic movements in which a number of distant muscles are engaged in simultaneous action. If the other associated

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[1] The dissection of Bendz, to which we have already referred, seem to show that these filaments of the accessory are distributed upon the larynx and œsophagus. They therefore probably assist in the movements of these parts.

movements can go on without it being necessary that the nerves supplying the muscles engaged in these should come from particular tracts of the spinal cord, then surely there can be no necessity for this in the case of the respiratory nerves. The insufficiency of Sir C. Bell's theory to answer the ends proposed has been pointed out in a most satisfactory manner by Dr. Alison in his elaborate Essay on Sympathy.[1] It appears to me that physiologists have been exceedingly premature in framing new systems of nerves to carry on the sympathetic and instinctive movements of the body: For I believe that it will be found, that all the nerves of the body which can transmit the influence of volition can also transmit the influence by which the muscles they supply are called into sympathetic movement; and that the reason of some muscles being called more frequently into sympathetic action than others, does not arise from any difference in the nerves supplying these muscles, but from a difference in the circumstances under which they are placed. We term the muscles of the extremities muscles of strictly voluntary motion, though we admit that when a person is falling forwards, the arms are instinctively thrown in front;—in other words, some of these muscles contract without our volition, and through means of an influence sent along the same nerves by which they are called into voluntary action. In the same manner, when the foot of an infant is pricked, the leg is instinctively drawn upwards; and the involuntary start of surprise is sufficient to show that the respiratory are not the only muscles of the trunk which are involuntary in their action. The respiratory muscles are said generally to be partly voluntary and partly involuntary; but it appears to me perfectly obvious, that they are as much muscles of voluntary motion as those of the

[1] Edinburgh Medico-Chirurgical Transactions, vol. ii.

extremities, and that the reason of their so frequently receiving the name of involuntary, arises from this, that, though properly voluntary, they are so much more frequently called into action by impressions accompanied by sensations than other voluntary muscles, that this has attracted the attention of physiologists to them in particular. We have seen that the muscles of the extremities (which every one calls muscles of strictly voluntary motion, to distinguish the mode by which they are stimulated to contraction from that of the heart and muscular coat of the intestines) perform associated movements without our volition on the excitation of certain impressions and sensations, and they obviously differ in no respect from the muscles of respiration, except in the relative frequency with which those involuntary movements are performed. This difference in the relative frequency of the action, again, evidently depends only upon the circumstances under which they are placed; for it is absolutely necessary for the preservation of the individual that the impressions arising from the want of fresh air in the lungs be frequently and regularly repeated, while the other sensations to which we have referred can be only occasional and accidental. It is obvious from these remarks that we do not object to the application of the term, muscles partly of voluntary and partly of involuntary motion, on account of its want of correctness, but because it is apt to mislead; for it may either induce us to believe that these respiratory muscles have at times some analogy in their mode of excitation to the heart and muscular coat of the intestines; or, what is more likely, that there is something specifically different between the nerves by which they are stimulated to contraction, and those distributed upon the muscles of the extremities. We believe that there are no muscles in the body which do not also sometimes deserve the name of involuntary, though the relative number of their involun-

tary movements may be far inferior to the strictly voluntary. If these views be correct, there appears to be nothing so peculiar in the action of the respiratory muscles that they should require any distinct set of nerves.[1] How, or in what manner the influences of volition and of certain impressions and sensations are invariably directed along certain motor nerves in preference to others, is at present, and probably ever will be, an utter mystery. The ends for which these sympathetic and instinctive actions were designed are obvious, but we have not been able to perceive any particular structural arrangement of the nervous system specially connected with their manifestation.

Though the sensitive and motor filaments of the spinal nerves appear to be separate from each other, and arise from distinct tracts of the spinal cord, yet it is not so with those arising from the upper part of this cord; and there appears to be some blending together of the motor and sensitive tracts of the spinal cord, when continued upwards into the *medulla oblongata*, which we cannot at present fairly explain. To say nothing of the *portio dura* and fourth pair, we find the *glosso-pharyngeal*, *par vagum*, and *spinal accessory* arising in the same line from the middle column of the spinal cord, and yet the filaments contained in these three nerves are partly sensitive, and partly motor; for no one can deny that the muscles of the *glottis* can and do frequently act from volition. It is possible that some of

[1] I do not mean to deny, that it is possible that the motor filaments for transmitting the influence of volition may be distinct from those which transmit that sympathetic influence by which muscles are called into action in obedience to impressions made upon distant parts, and that those two sets of filaments may be bound up intimately together in the nervous cords distributed upon the muscles of the trunk and limbs. What we maintain is, that the *nervous cords* supplying all the muscles of the trunk and extremities, are capable of transmitting both the influence of volition and of sympathetic movement; and that, therefore, there is no specific difference between the nervous filaments supplying the muscles of respiration and those of the extremities.

those motor filaments are furnished by the band of fibres which Mr. Solly[1] has lately described as passing across from the anterior column to the *crus cerebelli.* A good deal evidently still requires to be done before we can expect any satisfactory elucidation of the anatomical arrangement of these columns of the spinal cord, and their relations to the different nerves attached to them. We find, for example, Bellingeri and Sir C. Bell, two of the latest and best authorities on this subject, differ with regard to the origin of the posterior roots of the spinal nerves; for while Bellingeri[2] describes these posterior roots as arising partly from the posterior column, partly from the grey matter in the posterior collateral groove, and partly from the middle or lateral column, Sir C. Bell has renounced his former opinion, that they come from the posterior column, and now describes them as arising from the middle column.[3]

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[1] On the Human Brain, &c. [2] Oper. cit., p. 69.

[3] On the Nervous System, pp. 234 and 238, edit. 3d. Sir C., however, adds in a note, p. 234, that it is not impossible that the posterior column may be connected with the sensitive root of the spinal nerves, though he has not hitherto succeeded in tracing it.

No. V.

AN EXPERIMENTAL INVESTIGATION INTO THE FUNCTIONS OF THE EIGHTH PAIR OF NERVES, OR THE GLOSSO-PHARYNGEAL, PNEUMOGASTRIC, AND SPINAL ACCESSORY.[1]

PART SECOND.

(FROM THE EDINBURGH MEDICAL AND SURGICAL JOURNAL, APRIL, 1839.)

In a former Number of this Journal, (No. 134, January 1838, p. 109,) I detailed some experiments upon the three distinct nerves, generally denominated in this country the Eighth Pair, and also stated the conclusions which I believed may be legitimately deduced from them. I was obliged, for want of the requisite data, to defer the consideration of several important points connected with the functions of these nerves, and I have since that time been endeavouring to accumulate sufficient facts to supply some of those deficiencies. And as my remarks upon the functions of the pulmonary branches of the *vagus* were very cursory and incomplete, and as I did not even enter upon the consideration of the functions of the gastric branches of this nerve, I intend, in the present communication, to confine myself

[1] A short epitome of this paper was read at the meeting of the British Scientific Association, in September 1838.

chiefly to their examination. Before proceeding, however, with what may be considered as forming the principal object of this communication, I am anxious to make a few additions to and corrections of some of the statements contained in my former paper, which, in the order of arrangement, precede the remarks I have there made upon the pulmonary branches of the *vagus*.

Glosso-Pharyngeal Nerve.

In my former communication,[1] I stated, while making some remarks upon the reflex movements of the muscles of the throat and lower part of the face, observed on irritating the trunk of the glosso-pharyngeal, that I had "endeavoured in several of the experiments, by gently pricking, pulling, and pinching the nerve, to produce the usual muscular movements of deglutition," or "of those excited by disagreeable sensations in the fauces and pharynx, but without effect." In an experiment I performed last summer, a distinct effort of deglutition was made each time the trunk of the glosso-pharyngeal was irritated; and that under circumstances which, notwithstanding my numerous previous failures, induce me to believe that this was not an accidental coincidence, but actually stood in the relation of cause and effect. I shall briefly relate the facts of the experiments as they were witnessed by myself, so that every one may be able to judge how far I am justified in making this inference.

1*st Exper.*—The glosso-pharyngeal was exposed in a middle-sized terrier. The trunk of the nerve was pinched three times with the forceps, at intervals of two minutes, and each time a distinct movement of deglutition immediately followed. No such movement was observed when

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[1] *Vide* p. 83 of this volume.

the nerve was not irritated. The animal was now deprived of volition by a dose of prussic acid, and though the respiration went on for a short time, no effect followed the pricking of the glosso-pharyngeal, and the irritation even of the trunk of the *par vagum* appeared to have little effect in exciting the respiratory movements.

This last fact ought not, we conceive, to be considered as at variance with the first, for the prussic acid may, by its action upon the *medulla oblongata*, have rendered it considerably more obtuse to impressions conveyed by the glosso-pharyngeal, and the trifling effect which followed excitation of the *par vagum* may favour this view. Besides, it must be remembered, in judging of the functions of the glosso-pharyngeal as an exciter of deglutition, that impressions applied to the extremities of nerves generally act more powerfully than when applied to their trunks.[1]

Pneumogastric Nerves.

I have again had ample opportunities of confirming the statement made in my former communication—drawn from experiments upon dogs, rabbits, cats, and calves—that the pinching, cutting, and even the stretching of the *vagi* nerves, when exposed in the neck, are, in by far the greater majority of cases, attended by indications of severe suffering. In opposition to the opinion expressed by Dr. M. Hall and Mr. Broughton, that the *nervus vagus* is not a nerve of sensation, I adduced the authority of Haller, Brunn, Dumas, and Dupuy. If additional evidence be thought necessary, I may also add to those the names of Molinelli, Mayo, Magendie, and Brachet. In the 1st and

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[1] Volkmann has, from experiments upon the glosso-pharyngeal in the frog, arrived at the conclusion which we have from experiments upon the dog—that this is not a motor nerve.—*Vide* British and Foreign Medical Review, vol. vii., p. 237, January 1839; and Müller's Archiv. 1838. S. 85, 86.

5th Experiments, upon the *vagus*, related by Molinelli,[1] it is expressly mentioned that the animals (dogs) gave indications of suffering, in tying these nerves with a ligature. Mr. Mayo says, that " asses, cats, and dogs, almost invariably express great pain when this nerve, yet entire, is pinched with the forceps, and after its division equal suffering appears to result from pinching the part connected with the brain."[2] Magendie, in pointing out to his pupils an experiment where the nerve was stretched and cut without exciting pain, remarked—" In certain cases, on the contrary, the *nervus vagus* appears to possess the most exquisite sensibility; for it is scarcely touched without exciting immediately cries and convulsive motions."[3] Brachet in one experiment irritated the upper end of the cut *vagus*, with the view of subjecting the animal to suffering, and with success.[4] I attempted to give an explanation of the source of fallacy which had misled Dr. M. Hall and Mr. Broughton in their very limited number of experiments; but I am now convinced that there is another circumstance more likely to lead to such errors than the one I mentioned, and that is the very different degrees of sensibility possessed by different animals even of the same species. I have experimented on dogs which have endured the incisions necessary to expose the sheath of the carotid artery without any apparent uneasiness, and they remained quiescent, though the *vagus* was violently stretched, pinched, and cut. Magendie, in explaining an experiment upon the *vagi* made before his pupils, adds—" The degree of sensibility in the pneumogastric nerve is variable; the division

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[1] De ligatis sectisque nervis octavi paris. In Comment. Bonon., tom. iii., p. 280. Bononiæ, 1755.

[2] Anatomical and Physiological Commentaries, No. ii., p. 15. 1823.

[3] Leçons sur Les Phénomènes Physiques de la Vie, tom. i., p. 208. 1836.

[4] Recherches Expérimentales sur les Fonctions du Système Nerveux Ganglionaire, chap. i., Expér. 25. Paris, 1830.

of this nerve is sometimes followed by acute pain; and sometimes, on the contrary, the animal seems scarcely conscious of the operation."[1] Such facts ought to make us hesitate before arriving at negative conclusions upon the sensibility of nerves, and forcibly point out a serious error to which limited observations are liable.

In confirmation of an observation made by Dr. M. Hall and Mr. Broughton, I formerly stated that I had frequently repeated the experiment of compressing the *vagi* nerves in the neck, and that in some cases powerful respiratory movements were produced. As some additional confirmation of this observation, I may mention, that I remarked distinct respiratory movements apparently excited in three animals by compressing the *vagus* with the forceps, after they had been deprived of volition by a dose of prussic acid. This experiment, however, fails much more frequently than it succeeds.

In a note[2] I stated, that it is perhaps not quite correct to say, that the pharyngeal branches of the *par vagum* furnish all the motor nervous filaments of the pharynx and soft palate, and the more especially, as Palletta and Mayo had described, in the human species, a twig passing from the third branch of the fifth to the *circumflexus palati* muscle. With the view of enabling us to decide whether or not the branches of the fifth pair assist in moving the muscles of the soft palate, the following experiment was performed:—

2*d Exper.*—Three dogs were deprived of sensation and volition by small doses of prussic acid, and the skull-caps were sawn off as expeditiously as possible, the root of the fifth pair exposed on the cerebral side of the Casserian ganglion, and irritated by a powerful galvanic battery. Before, however, the galvanic wires were applied to the

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[1] Oper. cit., tom. ii., p. 234. 1837. [2] *Vide* p. 98 of this volume.

nerve, the cheeks and the greater part of the temporal and *masseter* muscles were rapidly divided, and the soft palate and *isthmus* of the *fauces* fully exposed. On irritating the nerve no movement of the muscles of the soft palate and *isthmus* of the *fauces* could be detected, though the *elevator* muscular fibres which remained attached to the lower jaw, acted so powerfully, that it required a strong effort to prevent its closure. After this had been repeated for a short time, the pharyngeal branch of the *nervus vagus* was exposed and irritated in two of these animals, both by the forceps and by galvanism, and distinct movements of the soft palate followed. In these experiments, we more than once supposed that the parts about the *isthmus* of the *fauces* moved on irritating the fifth; but were afterwards satisfied that this was mechanical, and dependent upon the convulsive movements of the muscles of the lower jaw; for when this bone was kept nearly fixed during the irritation of the nerve, the movements did not recur.

We believe, then, that in the performance of the function of deglutition the impressions are conveyed to the *medulla oblongata* along the branches of the glosso-pharyngeal; along the branches of the fifth pair distributed upon the *fauces;* and probably along those branches of the superior-laryngeal distributed upon the pharynx. The motive influence transmitted outwards from the *medulla oblongata* passes, we believe, along the pharyngeal branches of the *vagus;* along the branches of the hypoglossal, distributed to the muscles of the tongue, the thyro-hyoid, sterno-hyoid, and sterno-thyroid muscles; along the motor filaments of the recurrents ramifying upon the larynx; along some of the branches of the fifth, supplying the elevator muscles of the lower jaw; along the branches of the *portio dura,* ramifying upon the digastric and stylo-hyoid muscles and muscles of the lower part of the face; and probably along some of the branches of the cervical

plexus, which unite themselves to the *descendens noni.*[1] An examination of the various muscular actions engaged in deglutition will convince us that the muscles moved by the motor nerves we have enumerated, are employed in the performance of this function.

Laryngeal Branches of Vagus.—All the recent observations we have had occasion to make on these nerves while performing other experiments, have only served to strengthen the conclusions drawn from our previous experiments. We stated, in making a few remarks upon the pathology of *laryngismus stridulus*, that "from the experiments we have detailed, it is, however, apparent, that severe dyspnœa, amounting to suffocation, may arise both from irritation and compression of the inferior laryngeal nerves, or of the trunks of the pneumogastrics."[2] Without pretending to decide whether the disease we have referred to can, in many cases, or only in a few, be explained on Dr. Ley's theory, I would merely remark, that in some experiments to which I shall again have occasion to refer, I have witnessed, after the division of the *vagi* in the middle of the neck, and the consequent arrestment of the movements of the muscles of the arytenoid cartilages, sudden and violent attacks of dyspnœa, which generally went off in the course of a very few minutes, when they did not terminate in suffocation—phenomena which have been considered by some as affording an insurmountable objection to the theory of Dr. Ley, as it supposes the exciting cause of the disease to be permanent and not occasional. In these animals the respirations were performed with ease after section of both *vagi*, as long as they remained at rest,

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[1] We do not wish dogmatically to maintain, that the pharyngeal branches of the *vagus* may not include some sensitive filaments; but, we believe, on the grounds stated in the former communication, that they are almost, if not entirely, motor.

[2] *Vide* p. 120 of this volume.

or made moderate exertion; while, as soon as they began to struggle—in other words, when the inspiratory movements were performed with greater force—symptoms of suffocation presented themselves, and these in some cases shortly subsided, occasionally rather rapidly, on the animals refraining from the violent exertion. We have also, in some of these animals, designedly brought on a paroxysm of dyspnœa, by causing them to struggle.

I have lately had frequent occasion to experiment upon the inferior laryngeal nerves, and have observed that very few dogs give any indications of suffering when these nerves are irritated or cut. We are not, however, to conclude that they are entirely motor, for it must be remembered that many dogs remain perfectly quiescent, if previously well secured, during the incisions necessary to expose the nerve. Besides, the anatomical distribution of some of the filaments of the nerve, and the fact, that animals do occasionally give indications of suffering on irritating it, are sufficient to prove that it does contain some sensitive filaments.

Œsophageal Branches of Vagus.—Subsequent experiments on rabbits have furnished me with results exactly similar to those I formerly detailed under this head.[1] I have satisfied myself, however, that substances seem to pass pretty freely along the *œsophagus* in most dogs after section of the *vagi*. Upon what this difference depends I have not yet been able to form any probable opinion.

Cardiac Branches of Par Vagum.—Brachet relates an experiment, in which he states, that he tortured an animal (dog) in various ways, after having previously divided the *vagi*; and although it manifested by its struggles and cries the pain which it suffered, yet the heart's action was not

[1] *Vide* p. 121 of this volume.

quickened, "le cœur est resté impassible, ses mouvemens n'ont pas varié."[1] This statement is so much at variance with all we know of the physiology of the heart, that we might at once declare that some oversight must have been committed. The movements of the heart are distinctly referrible to the same laws that regulate muscular contractility in other parts of the body, only somewhat modified, to adapt it to the performance of its appropriate functions. Like all the other muscles of the body, it is endowed with the property of irritability, which enables it to contract upon the application of a stimulant; and whether we embrace the opinion that muscular irritability is dependent upon nervous influence, or adopt the much more probable doctrine that it is a property of the muscular fibre itself, it is sufficiently proved—and this is admitted by M. Brachet himself—that the irritability of the heart is not derived from the *vagus*. The ordinary and habitual excitant to the irritability of the heart is the blood constantly flowing into its cavities. When the blood is forced on more rapidly towards the heart—as in exercise, its contractions become proportionally more frequent; and when the current moves on more slowly—as in a state of rest, its frequency becomes proportionally diminished. If the contractions of the heart were not dependent upon the blood, and their number not regulated by the quantity flowing into its cavities, very serious and inevitably fatal disturbances of the heart's action would soon take place. As statements such as those of M. Brachet are, however, more effectually met by facts than by arguments, I proceeded to put them to the test of experiment. These experiments were seven in number, and six of them were made in the following manner:—The *vagi* and sympathetics—and in some cases

[1] Opus cit., chapitre premier, Exper. 25.

the recurrents also, were cut in the middle of the neck, and a portion of each removed. At a longer or shorter period after the operation the pulsations of the heart were reckoned when the animal was lying or standing on the ground, and after it had been caressed for some time to calm its fears. It was then lifted up on the table on which it had been previously tied and operated upon, and after having been spoken to harshly, the pulsations were again reckoned. After being again caressed for some time, the pulsations were counted a third time; and when replaced upon the ground, they were reckoned a fourth time. The following results were obtained:—In the first dog the pulsations of the heart were about 140 before the commencement of the experiment. The animal at this time was apparently somewhat alarmed. Four hours and a-half after division of the nerves, the pulsations of the heart were about 170 when the animal was standing on the ground, and rose to 200 at least when placed upon the table. After it was replaced on the ground they had again fallen to about 170. After nineteen hours the pulsations were 160 on the ground; they rose again to about 200 when placed on the table—again fell to about 160 when still on the table, and were not increased by being replaced on the ground. In the second dog the pulsations were 156 on the floor, and about 190 on the table; and in the third dog they rose 20 beats in the minute when placed on the table. In both of these two last experiments the pulsations of the heart soon subsided to their former frequency, and were not increased by replacing the animals on the ground. In the fourth dog the pulsations, twenty-four hours after division of the nerves, were 140 on the floor, and instantly rose to 180 on the table. After waiting until they had again fallen to their former frequency, they were not increased by replacing it on the ground. In the sixth dog the pulsations were 140 the third day after the section of the nerves, when the animal

was on the floor, and were raised to 160 by placing it on the table. In these experiments it was particularly oberved, that the animals made no struggles in carrying them to and from the table, and, consequently, the increased excitation of the heart must have arisen from the mental emotion of terror. In the seventh dog this was conjoined with violent struggles. The pulsations—eight hours and a-half after the operation, were 130; when placed on the table, and made to struggle, the pulsations, as far as could be made out, were about 220; when he had been subjected to pain, and had struggled more violently, they became so frequent that they could not be accurately reckoned, but were at least 260 in the minute. A large tube had been previously introduced into the *trachea* in this last animal. These experiments are, we conceive, sufficient to prove that, after section of the *vagi*, the pulsations of the heart may not only be quickened by muscular exertion, but also by mental emotions. Though in all probability the *vagi* are the usual channels through which mental emotions affect the heart, yet it appears from these experiments that this may also take place through the medium of the ganglionic system of nerves.

Pulmonary Branches of the Vagus.—In my former paper[1] I gave the results of several experiments, from which, in opposition to the observations of Magendie, Wilson Philip, and Swan, I concluded "that lesion of one of the pneumogastrics does not necessarily or even generally induce disease of the lung of that side." Since that time I have carefully examined the lungs of two dogs and a cat, killed some time after a portion of one *vagus* had been removed. One dog lived two months, the other nine days, and the cat three weeks. No morbid change could be detected in the lungs. I have now removed a portion of one *vagus* in seventeen

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[1] *Vide* p. 133 of this volume.

animals, which have been allowed to live a longer or shorter period—from twenty-four hours to six months, and in none of these could I detect any morbid change in the lungs that could be attributed to the section of the nerve. In an experiment made by Magendie before his pupils, the results were completely at variance with his former expressed opinions. The right lung of a dog, from which a portion of the *vagus* of that side had been removed six months before, was on examination found to be perfectly healthy.[1] The circumstance of the lung remaining healthy after section of the *vagus* of the same side might, I formerly supposed, be accounted for by the anastomoses between the pulmonary plexuses of the two sides. I now believe, however, that there is another cause in operation which I shall have to discuss at some length when we come to examine the morbid changes in the lungs induced by lesion of both *vagi*.

Effects of Lesion of the Par Vagum upon the Respiratory Muscular Movements.—It has now been fully ascertained by numerous experimenters, more especially by those who have investigated the functions of the *vagus* since the time of Legallois, that an animal will continue to breathe for a longer or shorter time, after this nerve has been cut on both sides of the neck, if care be taken to secure the free egress and ingress of air into the lungs. In my former communication I examined, under the section *Laryngeal Nerves*, the effects of lesion of the *vagi* in the middle of the neck upon the respiratory muscular movements of the *larynx*, and it will be here unnecessary to resume the discussion of that point. I there adduced various facts to show that when the *vagi* are injured above the origin of the inferior laryngeal or recurrents, the movements of all the muscles which enlarge or diminish the cavity of the *larynx* are arrested, and the

[1] Leçons sur les Phénomènes Physiques de la Vie, tom. i., pp. 203, 204. 1836.

superior aperture of the *larynx* can no longer be dilated during inspiration. If the *larynx* be large, and the animal refrain from any violent effort, an adequate quantity of air may still find its way through the diminished aperture of the *larynx*, and the respirations be at first performed with ease. If, on the other hand, the *larynx* be small, the quantity of air admitted through the interior of the *larynx* may be insufficient to carry on the respiratory process, and the animal may labour under dyspnœa from the moment the nerves are divided until its death. In young animals, in which the *larynx* is naturally small, and even not unfrequently in those full-grown, especially if the respiratory movements become forcible as in struggling—or, in other words, when the capacity of the chest is suddenly and greatly enlarged—the air rushes through the diminished aperture of the *larynx* in a narrower stream and with increased force, carrying the arytenoid cartilages mechanically inwards, producing the complete occlusion of the superior aperture of the *larynx*, and thus suffocating the animal.

As it becomes a matter of importance, in enabling us to arrive at some accurate conclusions upon the cause of the morbid changes induced in the lungs by lesion of the *vagi*, to ascertain the immediate effects of this operation upon the respiratory movements, I have of late attended particularly to this point, and have notes of thirty experiments in which the immediate effects of division of the *vagi* were watched upon dogs. In a considerable number of these the recurrents were also divided. Seventeen of the experiments were performed chiefly for the purpose of investigating the morbid changes in the lungs. In the others I had different objects in view, and some of the animals were therefore allowed to live only a short time after the operation, but sufficiently long to ascertain its immediate effects upon the respiration. I need only state once for all, that in every one of these experiments the nerves were not simply divided,

but a considerable portion was also removed. Many of the animals operated on were full-grown, so that no opening was made into the *trachea*. In others which were not full-grown a large tube was introduced into the *trachea* previous to the division of the nerves; and in a few in which (although they were full-grown) violent paroxysms of dyspnœa occurred after the nerves had been divided, an opening was immediately made into the *trachea* and a tube introduced. Such experiments are liable to several sources of fallacy that must be carefully avoided, if we wish to arrive at accurate results. When an opening is made into the *trachea* under such circumstances, and no tube introduced, the incision in the skin and muscles must be very free, to secure a direct communication between the opening in the *trachea* and the external air, as the animal generally droops his head and the *trachea* is movable. If a tube be introduced into the opening, care must be taken that it be sufficiently large, and not clogged up by blood or mucus, and that its shape be such as to prevent the orifice being obstructed in the bent position of the neck. Reflecting upon these inconveniencies and sources of error, it occurred to us that by selecting aged animals, and after first ascertaining what effect the section of the recurrents had upon the supply of atmospheric air in the particular animal operated on, we might be enabled to judge more accurately of the extent of the influence of the *vagi* upon the other respiratory muscular movements. We believed that by selecting those animals in which the division of the recurrents produces no impediment to the respiration, the subsequent division of the *vagi* would cause no additional diminution of the supply of air through the *larynx*, and would consequently determine in a satisfactory manner the immediate effects of the lesion of the *vagi* upon the respiratory movements, under circumstances where the egress and ingress of air to the lungs were quite free. Another difficulty, however, again

presented itself, to which I have already alluded. In some of those animals the violent respiratory movements produced by the pain of dividing the *vagi*, or some powerful effort to liberate themselves before they were unbound, produced violent dyspnœa, which in some cases soon went off when the animal became more quiescent; but in others necessitated the opening of the *trachea* and the introduction of a tube. The idea of dividing the right *vagus* below the origin of the inferior laryngeals now occurred to us, but this being a severe and difficult operation was after one trial abandoned. It appeared, then, that the best plan to follow was to select full-grown animals; to avoid as much as possible any thing likely to produce the struggles of the animal, after or during the division of the *vagi;* and if dyspnœa, threatening suffocation, should occur, quickly to introduce a pretty long and large bent tube into the *trachea*, and efficiently secure it. These details may appear uninteresting, and to many may seem unprofitable; but it is obvious that those who may wish to test the accuracy of the facts I am about to mention, must repeat the experiments in a similar manner, unless it should appear that I have overlooked the agency of some extraneous circumstance that might interfere with the results. It is not so much the frequency of false observations that we have to complain of in medical science, as the errors which arise from making these observations under dissimilar circumstances, and from drawing general conclusions from insufficient data.

Of the thirty animals operated on, I find from my notes that fourteen, or rather less than one-half, had the *trachea* opened. Of these fourteen the *trachea* was opened, and a tube introduced in nine, previous to the cutting of the *vagi*, as the age of the animal or the dread of the supervention of suffocation in some other subsequent experiment to which the section of the *vagi* was preparatory, made it prudent to proceed in this manner. In the remaining five,

the *trachea* was opened subsequently to the commencement of the severe dyspnœa—in four of these after section of the *vagi*, and in one after section of the recurrents, and before the *vagi* had been exposed. Of these thirty animals, I find that the respiration was easy immediately or soon after the division of the *vagi*, and this continued for a longer or shorter time in twenty-seven; while in the remaining three it is stated to have been difficult and approaching to dyspnœa. The *trachea* had been opened in these three animals, and in one of them, as no sufficient tracheotomy tube was at hand, none was introduced. In the other two, tubes were introduced; but I have every reason to believe that they answered the purpose very imperfectly. Besides, it ought also to be mentioned, that these three experiments were performed before I had matured my plans for providing for the passage of a sufficient supply of air through the *trachea*. I may also state here, that I could adduce several experiments, performed last year, which would seem to show that dyspnœa may occur immediately after section of the *vagi*, even where precautions have been used against it; and which at the time almost induced me to adopt the opinion, that the synchronous movements of the muscular fibres of the bronchial tubes were essentially necessary for healthy respiration. In reflecting upon the somewhat imperfect manner these experiments were performed, and finding that they are not confirmed by the twenty-seven experiments where all apparent sources of fallacy were avoided, I have arrived at the conclusion, that when a sufficient quantity of air traverses the *trachea* after section of both *vagi* and recurrents, the respirations are at first performed with ease. Very important effects, however, upon the respiratory movements manifest themselves instantly after section of the *vagi*. The frequency of the respiratory movements suffers a great diminution, they are at the same time performed more slowly, and generally even

from the first in a somewhat heaving manner. I have notes, more or less complete, of the frequency of the respirations taken at different periods after section of the *vagi* in twenty dogs. We found it frequently difficult, and in some cases impossible, to reckon the number of respirations accurately in some of these animals previous to the commencement of the experiment, owing to their restlessness, and the gentle manner in which their respiratory muscular movements were performed; and this will explain some deficiencies in the following tables. We were also sometimes not a little perplexed by the varying frequency in the number of the respiratory movements in some of those animals, even within the space of a few minutes, and when no very apparent change had occurred in the circumstances in which they were placed. After the *vagi* had been cut, we experienced much less difficulty in reckoning the number of the respirations, as they were performed in a more slow and heaving manner. In the following Table, I shall not arrange the experiments in the order they were performed, but classify them in the way that appears best adapted to show the results. The first Table is for the purpose of pointing out the difference between the number of respirations before and at various periods after section of the *vagi*, and in the cases which I shall afterwards mention, of the recurrents also. I may state that the observations were made in this manner: The respirations were reckoned several times immediately before the operation was performed—with the exception of the 11th Experiment, where the animal was repeatedly examined for two days previous—and then again immediately after the division of the *vagi*, and before the incisions were stitched up. The T in the tables indicates that a tube was introduced into the *trachea*. I have marked at the end of some of the experiments the time the animal lived after division of the nerves, as we shall afterwards have occasion to refer to these.

TABLE I.

Number of Experiments.	Number of Respirations before Operation.	Number of Respirations immediately after Operation.	Number of Respirations at various periods after Operation.																		
			Minutes.					Hours.						Days.							
			2	5	10	15	25	2	4	5	6½	8	11	1	2	3	4	5	6	7	12
3 T.	15	6-7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
4	14-16	5-6	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
5	18-20	7-8	8	7-8	...	8-9	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
6	16-17	6-7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
7	16-18	8	...	...	12	7	7·8	...	...	...	...	...	...	...	...	...	...	...	...	...	...
8 T.	24	8	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
[1] 9	24-28	14	...	...	12	...	...	...	14	...	...	...	...	...	...	...	...	...	...	...	...
[2] 10	16	8	...	...	...	...	...	9	...	...	...	9	...	7	7	...	7	7	7	7	...
[3] 11	10-16	8	...	...	...	...	...	6-7	...	...	...	...	...	6	...	6	5	5	4-5	5	...
[4] 12	20-26	7-8	...	...	...	...	...	8-13	...	...	...	...	...	...	...	...	...	...	...	...	...
[5] 13	16	8	...	...	...	...	...	...	...	7	7	...	7	8	8-12	7-8	7	7	7	7	7

[1] Died nineteen hours after the operation.
[2] Died on the eighth day.
[3] Also died on the eighth day.
[4] Died on the third day.
[5] Was killed on the twelfth day.

TABLE II.

Number of Experiments.	Number of Respirations before Operation.	Number within ten minutes after Operation.	Number of Respirations at various other periods after Operation.																	
			Hours.									Days.								
			½	2	4½	5	8	9	10½	19	28	1	1¼	2	3	5	6	7	8	9
[1] 14 T.	16	...	...	...	7	...	7	...	7	...	...	...	...	...	...	...	...	...	...	...
[2] 15	16	...	14	...	...	...	...	...	...	9	12-20	...	...	9	10	12	12	10	16	7
[3] 16	12	8	...	8	...	7	...	7	...	...	..	10	9	10	...	...	...	...	...	...
[4] 17 T.	16	20	...	...	8	...	6	...	...	...	...	8	8	...	...	...	...	...	...	...
18 T.	16-18	...	...	...	...	...	6	...	...	...	...	8-10	...	...	...	...	...	...	...	...

[1] Died twenty-four hours after the operation.
[2] Died on the twelfth day.
[3] Died on the third day.
[4] Died thirty-four hours after the operation.

TABLE III.

Number of Experiments.	Number of Respirations at various periods after Operation.														
	Soon after.	Hours.									Days.				
		$\frac{3}{4}$	4	5	7	8	12	15	19	21	1	2	$2\frac{1}{4}$	3	$3\frac{1}{2}$
[1] 19	...	14	7	...	...	...	...	...	7	11	...	...	..	...	...
[2] 20	7	...	...	...	...	...	...	...	...	...	7	6	...	...	...
[3] 21	...	...	...	...	12	...	...	8	...	...	12	...	...	...	...
[4] 22	...	...	...	8	...	...	...	...	...	...	10-12	...	...	6-7	...

In examining the above tables it will be at once apparent in comparing the number of respirations immediately before and immediately after section of the *vagi*, that a very considerable diminution of the number of the respiratory muscular movements followed the division of these nerves, with the exception of the 15th and 17th Experiments. It is stated in my notes, and the varying number of the respirations sufficiently indicates the fact, that the respiration was very easily excited in the animal the subject of the 15th Experiment. And as this was not remarked until the nerves had been divided, no safe conclusions can be drawn from such an experiment, especially as the respirations were not reckoned sufficiently often before the commencement of the experiment to ascertain their average frequency. The result of the 17th Experiment is so com-

[1] Died at the end of twenty-four hours.
[2] Died at the end of three and a-half days.
[3] Died after fifty-four hours.
[4] Died at the end of the third day.

pletely at variance with the others, that the circumstances connected with its performance require some explanation. This animal was one of those in which it was necessary to open the *trachea*, and introduce a tube after section of the *vagi*. It had suffered much from dyspnœa before the tube could be introduced, and the respirations were reckoned soon after the insertion of the tube, and before it had recovered from the effects of the dyspnœa. I may also state, that this was the first experiment I made to ascertain the effects of the division of the *vagi* upon the frequency of the respiratory movements, otherwise I would have paid more attention to this circumstance, and would have again reckoned the respiration immediately after the animal had become calm. This experiment ought, perhaps, to have been rejected from the list, but I was anxious to give all the observations made on this point exactly as they are entered in my notes, so that every one may be able to judge of the soundness of the conclusions we intend to deduce from them in a subsequent part of this paper. In judging of the effects of the division of the *vagi* upon the frequency of the respiratory movements, it is absolutely necessary to reckon them; for as these movements become more apparent, prolonged, and heaving, we may be readily deceived. From neglecting this, I fell, in my earlier experiments upon the *vagus*, into the error of supposing that the respiratory movements were increased in frequency, especially in rabbits. It is in this manner that we can account for the statement of Dr. M. Hall, that after division of the *vagi* " the acts of respiration immediately become much more frequent."[1] At a shorter or longer period after section of the *vagi*, the respirations become more heaving and prolonged, while the expirations continue to

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[1] Memoirs on the Nervous System, p. 85. 1837.

be comparatively short and rapid, and frequently attended by a sound caused by the sudden expulsion of the air.

In some cases the number of respirations was afterwards still farther reduced; while in others it varied at different periods; and in a few it became more frequent shortly before death. At a longer interval after section of the nerves, the inspirations generally become still more heaving and prolonged; the blood is less perfectly arterialized in the lungs, and the arteries circulate blood gradually approaching to the venous character; the animal becomes more dull and stupid, the evolution of animal heat is diminished, and it dies asphyxiated.

We believe that it is sufficiently evident from these experiments, that the *vagi* are important nerves in transmitting those impressions to the *medulla oblongata* that excite the respiratory muscular movements; and they may appear to favour the opinion of Dr. M. Hall, that the continuance of the respiration, after division of the *vagi*, is a voluntary and not an excited act.[1] In my former communication, (p. 161,) I stated that I had observed respiratory movements continue in animals after they had been deprived of all volition by a small dose of prussic acid. I have since made similar experiments upon animals deprived of volition by alcohol, and by blowing air along the carotids towards the brain. I am also satisfied that, in such experiments, the inferior laryngeal nerves may be also divided without affecting the results. As, however, it has been stated by Dr. M. Hall, on the authority of Cruveilhier, (Lancet, February 17, 1838, p. 733,) that after the function of the cerebrum is destroyed, or, in other words, after the animal has been deprived of volition, the respiration ceases instantly on dividing the pneumogastrics

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[1] Oper. cit., p. 87.

near their origin, I thought it necessary to repeat this experiment.

23d Exper.—The *vagi* were first exposed in the upper part of the neck in a puppy six days old. A considerable portion of the *cranium* was removed on both sides of the superior longitudinal sinus, and the hemispheres of the brain were completely destroyed down to the *corpora quadrigemina.* The *vagi* were then cut, and the *trachea* opened, without arresting the respiratory movements. The animal was evidently much exhausted from loss of blood, and the respirations were performed at long intervals even before the *vagi* were divided. After the section of the *vagi* the respiration went on for several minutes, during which the *cerebellum* was also broken up by the forceps.

24th Exper.—The *vagi* were exposed and the hemispheres of the brain removed, as in the preceding experiment. The respirations continued after the division of the *vagi.* A few minutes after division of the nerves, the respirations were reckoned and found to be four in the minute. The recurrents were then divided, and the *cerebellum* broken up as cautiously as possible by the forceps. Seven minutes after the respirations had been first reckoned, they were between three and four in the minute; at the thirteenth minute they were still between three and four. The spinal column was completely severed between the second and third cervical vertebræ, fifteen minutes after the respiration had been first reckoned, and the respiratory movements still continued at the rate of one in the minute. At the twentieth minute the head was entirely removed from the body, and some cold water was dashed over the face after it had lain quiescent for nearly two minutes, and the muscles of the face moved as in inspiration. In about two minutes more, a similar movement followed without the artificial application of any external excitant. Between the seventh and thirteenth minute after the respiration had

been first reckoned, the animal repeatedly sucked the finger introduced into the mouth, and at the fifteenth minute it still did this, though feebly.

These two experiments being positive ones are sufficient, independent of others we could adduce, to prove that the respiratory movements are not immediately arrested after removal of the *cerebrum* and *cerebellum* and division of the *vagi*. We have seen that division of the *vagi* in an animal whose *encephalon* was uninjured was immediately followed by a diminished frequency of respiration; and it becomes an interesting question to ascertain what is the effect of the lesion of those nerves in an animal deprived of volition by the previous removal of the brain, as such experiments will determine the influence of the *vagi* as exciters of respiration in a more precise manner than those performed upon animals whose respiratory movements may be readily modified by volition.

25*th Exper.*—The pneumogastrics were exposed and surrounded by a loose ligature in a kitten one day old. The hemispheres of the brain were removed, and attempts were made to break down the *cerebellum* without removing the osseous *tentorium*. Before the commencement of the operation the respirations were nearly one hundred in the minute; after removing the hemispheres of the brain they were about forty. On cutting the *vagi* they instantly fell to between three and four in the minute, and the animal continued to breathe in this manner for an hour, when it was left. During this time it sucked the finger introduced into the mouth.

Another experiment of the same kind was performed on a kitten of the same age, whose respirations were also about one hundred in the minute. This animal appeared to be more exhausted from hemorrhage, and only breathed twelve times in a minute after the removal of the hemispheres. After the section of the *vagi* the respirations were

only between two and three in the minute; and it continued to breathe in this manner for a quarter of an hour, when it was left. On examining these two animals after death, it was ascertained that the whole of the *cerebrum* down to the *corpora quadrigemina* had been removed, and that the *cerebellum* was only partially injured. I find it stated in my notes of these two experiments, that, if the animals had been left immediately after section of the *vagi*, we might have gone away with the impression that they had ceased to breathe, as the respiration appeared quite arrested for about a minute.

26*th Exper.*—The *vagi* and recurrents were exposed, the hemispheres of the brain removed, and the *cerebellum* injured, in a kitten five days old, as in the previous experiments. The respirations before the commencement of the experiment were 120; after removal of the hemispheres of the brain 40; after section of the *vagi* and recurrents they instantly fell to four in the minute; two minutes after this they were between three and four; five minutes after they were four; and seven minutes after they were still four; It was now pithed at the lower part of the neck, between the sixth and seventh cervical vertebræ, and the respirations fell to one in the minute. It continued to breathe in this manner for seventeen minutes, when it was left.

A similar experiment was performed on another kitten of the same age. The respirations were few and irregular after the removal of the cerebral hemispheres. The *vagi* were then cut, and it continued to breathe about once in the minute for half an hour, when it was left. On dissecting these two animals after death, it was found that a considerable part of the *cerebellum* had escaped injury in the former; while in the latter, nothing except the *medulla oblongata* had been left within the cranium.

While these experiments illustrate the great importance of the pneumogastrics as exciters of respiration, they also

prove that the impressions made on the filaments of the pneumogastrics distributed in the air-cells of the lungs are not the sole excitants of respiration; and that there are other nerves which can transmit impressions to the *medulla oblongata* capable of exciting the involuntary respiratory movements.

I formerly suggested (p. 136) that the continuance of the respiration after section of the *vagi* might depend upon the transmission of impressions along the sympathetic branches distributed in the lungs, backwards to the spinal cord. Several facts related in the above experiments upon the effects of the division of the *vagi* after removal of the brain seem to prove that all the impressions conveyed to the *medulla oblongata* capable of exciting the involuntary muscular movements under such circumstances, are not transmitted through the sympathetic system. I need only refer to the gasping of the animal after decapitation—a fact formerly observed by Legallois—and to the continuance of the respiration after the pithing of an animal in the cervical region, from which the brain and *cerebellum* have previously been removed, and the *vagi* divided high in the neck. I may also state, that I have seen the respiratory muscles twice called into simultaneous movement after all the thoracic viscera had been rapidly removed in an animal deprived of sensation and volition by a dose of prussic acid. The facility with which the respiratory movements are excited by impressions made upon the sensiferous filaments of the fifth pair—well illustrated by the effects of dashing cold water on the face—will at once suggest this nerve as a likely channel through which these impressions may be conveyed. The effects of certain impressions upon the surface of the skin generally, and the details of two very interesting and important cases of the resuscitation of new-born children by the contact of the cool atmosphere with the surface of the body, given by Dr. M. Hall, on the

authority of Dr. Heming,[1] and by Dr. Wagner,[2] point out the filaments of the spinal nerves distributed on the skin as also probably engaged in transmitting those impressions which excite respiratory movements. It would be difficult to devise experiments likely to afford such decisive results as would enable us to ascertain the relative share which these different nerves have in carrying on the respiration after section of the *vagi*. The results of the 23d and 25th experiments, where the respiratory movements suffered another very apparent diminution on pithing the animal in the cervical region, would seem to show that the nerves distributed about the face and upper part of the neck do not certainly furnish the only channels through which the impressions which excite respiration are conveyed to the *medulla oblongata* after division of the *vagi*. We must, however, be cautious in drawing conclusions from negative experiments so few in number. With the view of throwing further light upon this question, the following experiment, which is similar to one detailed by Mr. Cruickshank,[3] was performed.

27*th Exper.*—The *vagi* and sympathetics were divided low in the neck in a powerful bull-bitch, at eight o'clock, A.M. A large tube was introduced into the trachea before the division of the *vagi*. The respirations before the operation were from sixteen to eighteen in a minute. After the operation it breathed freely through the tube and walked about. At four, P.M., the respirations were six in the minute, when it was lying at rest. Next morning, at seven o'clock, the respirations were reckoned repeatedly, and were found to be from six to seven when lying, and from eight to ten when standing. It was now pithed at the lower part of

[1] Memoirs on the Nervous System, p. 88.

[2] British and Foreign Medical Review, vol. v., p. 582.

[3] Medical Facts and Observations, vol. vii., Experiment vii., p. 147.

the neck, and it continued to breathe by the diaphragm alone, and appeared uneasy during the short time it was allowed to live. The respirations were reckoned between two or three minutes after the spinal cord had been cut across, and were six in the minute. For twenty minutes after this they were reckoned at short intervals, and were still six in the minute. Both femoral arteries were now opened, and the blood, though of arterial hue, was darker than usual, and flowed out with diminished impetus. No satisfactory conclusions can be drawn from this experiment, as the frequency of the respiration was probably influenced by volition and other causes. As far as a single experiment of this kind can go, however, it appears to show that the frequency of the respiratory movements was somewhat diminished by the pithing of the animal, for I am convinced that the respirations would have been more frequent had the animal been as much excited before it was pithed as it appeared to be afterwards. In Mr. Cruickshank's experiment the animal died sixteen hours after the operation. The number of respirations varied from five to fifteen in a minute. Their frequency before the operation is not stated.

The diminution of the frequency of the respiratory movements consequent upon division of the *vagi* has been observed by other experimenters. A. G. F. Emmert concluded, but apparently more upon theoretical grounds than from any direct observations made in the two experiments he had at that time performed on rabbits, that after lesion of the *vagi* the respirations became less frequent and prolonged.[1] Mayer reckoned the number of respirations, both before and at various periods after section of the nerves, in

[1] Archiv für Physiologie, von Reil und Autenrieth. Neunter Band, S. 417. 1809.

five experiments, and gives the results in the following table :—

	Before Operation.	After Operation.
Ass,	17	8
Dog,	48	8
1st Rabbit,	80	40
2d do.	100	48
3d do.	80	28[1]

The late Mr. Broughton mentions, that in a horse whose *vagi* were divided, "the respirations became slow, twelve in a minute."[2] In another horse, "the respirations fell to five in the minute."[3] At what period after the operation this diminution in the frequency of the respirations was observed, and what was the number of the respirations previous to the experiment, we are not informed. Sir Astley Cooper has given the results of two experiments upon rabbits, which well illustrate the effects of section of the *vagi* upon the frequency of the respiratory movements. In one rabbit the respirations were 132 in the minute before the commencement of the experiment. One hour after division of the nerves they were forty-eight; and after eleven hours and a-half they were only thirty. In the second experiment the respirations were 135 before the division of the nerves. One hour after, they were forty-eight; six hours after, they were thirty-six; and after sixteen hours they were twenty-eight.[4] The experiments we have related prove that this diminished frequency of the respiration instantly follows the division of the nerves, and cannot depend upon any blunting of the sensation from the circulation of dark blood in the arteries of the brain; though this no doubt may, after a while, probably assist in

[1] Tiedemann's Zeitschrift für Physiologie. Zweiter Band, S. 77. 1826.
[2] Quarterly Journal of Science, &c., vol. x., p. 305.
[3] Oper. cit., p. 307.
[4] Guy's Hospital Reports, vol. i., pp. 469, 654. 1836.

diminishing them still farther. We shall have occasion to examine the probable influence of the diminished frequency of the respirations in producing the morbid changes observed in the lungs, when we come to that part of our inquiry.

Are the Pulmonary Branches of the *Vagus* both Motor and Sensitive Nerves? We have made various attempts to obtain some satisfactory evidence that the muscular fibres of the bronchial tubes are moved through the influence of the *vagi*, but we must confess that these as yet have all failed. I have frequently performed the following experiment upon animals immediately after death. An opening was made into the *trachea*, into which the broad end of a bent tube of the form of a blowpipe was inserted and there secured, while the narrow end was introduced below the surface of water in a shallow vessel. The nerves were then immediately exposed, and irritated both by the scalpel and by galvanism; and we anticipated, that, if the muscular fibres of the *bronchi* were thrown into contraction, a part of the air would be forced along the tube, and rise through the water in the form of bubbles. No such appearance, however, presented itself, though the muscular fibres of the œsophagus moved at each application of the excitant. We do not, however, consider this a very delicate method of testing this point, from some circumstances we observed in performing these experiments. In one or two cases in which I believed I heard the natural respiratory murmur after section of the *vagi*, I could detect no change in the respiratory sound when the lower end of the cut nerve was pinched by the forceps during inspiration. The restlessness and tremblings of the animal generally, however, interfere with the accuracy of such observations. We certainly do not consider these negative experiments as by any means sufficient to entitle us to conclude that some of the pulmonary branches of the *vagus* are not motor

nerves—the more especially as the well-known symptoms of what is called spasmodic asthma, and certain phenomena observed by Laennec on applying the stethoscope over the chest,[1] render it exceedingly probable that muscular movements occur in the bronchial tubes; and if such movements be influenced by nerves, it is much more likely that these should belong to the *vagus* than to the sympathetic system. M. Brachet has related some experiments which, if correct, prove in a most satisfactory manner that part of the pulmonary branches of the *vagus* are motor.[2] We have not repeated those experiments; but, from an examination of the details given by M. Brachet, we cannot help suspecting that some error must have been committed in their performance, which had been overlooked.

From the distribution of the *vagus* upon the bronchial tubes we might conclude *à priori*, that it must be concerned in conveying those impressions to the central organs of the nervous system which excite sensations. It does not, however, necessarily follow that division of the *vagi* should annihilate all the sensations referred to the lungs. I formerly stated, that I was convinced, from frequently repeated experiments, that section of the *vagus* on both sides neither arrests the transmission of those impressions to the *medulla oblongata* which excite the involuntary respiratory movements; nor does it annihilate the sense of anxiety arising from the deficiency of fresh air in the lungs. I have since that time had frequent opportunities of verifying this statement, and I am confident that animals will evince great uneasiness—to judge from the struggles of the animal—after section of these nerves, when the access of air to the lungs is prevented. It is possible, however,

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[1] Treatise on the Diseases of the Chest, &c., pp. 418, 419, Forbes' Translation. 1829.

[2] Opus cit., p. 299, Expériences 125, 126.

that this *besoin de respirer* may be diminished by section of the *vagi*, though it certainly is not annihilated. In order to perform such experiments properly, it is necessary that the access of air to the lungs be fully and suddenly prevented. Such experiments as those performed by Brachet[1] and Mr. Grainger,[2] where the animal was placed under a bell-glass in a limited quantity of air, are liable to an obvious source of fallacy. When carbonic acid is accumulated slowly in the air inspired even in a healthy individual, a torpor gradually creeps over the nervous system, and the sense of anxiety at the chest is never very urgent, and frequently not sufficient to rouse a person from his sleep. The melancholy fate of many an unfortunate individual could be easily adduced in confirmation of this. I have seen rabbits confined under a bell-glass until their breathing became quick and heaving, without their evincing any marked uneasiness, and without making efforts to escape. The other method of experimenting followed by Brachet, viz., of cutting the *vagi* in a puppy three days old, and then placing the nose under water,[3] is equally liable to error. From the various experiments we have made, we are led to believe that section of the *vagi* in an animal so young is followed by the instant occlusion of the superior aperture of the *larynx*, so that the struggles and uneasiness arising from the occlusion of the *larynx* may have ceased before the nose of the animal was placed under water. At all events, we found on repeating this experiment on kittens, that plunging the nose in water was a very useless proceeding, for violent struggles and evident uneasiness instantly followed the division of the *vagi*.

In our last communication (p. 140) we alluded to the

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[1] Opus cit., p. 299, Expérience 37, p. 134.

[2] On the Spinal Cord.

[3] Opus cit., Expérience 34.

experiments made by Brachet, from which he concludes that all the sensations occasioned by foreign bodies, &c., in the air-passages are dependent upon the integrity of the pneumogastrics. We stated that we had never yet been able to induce the severe paroxysms of coughing described by Brachet, by any mode of irritating the inner surface of the *trachea* we have adopted. To show that others have also sometimes observed great insensibility of the mucous membrane of the *trachea*, even when the *vagi* were entire and uninjured, I have only to refer to the works of Haller.[1] He there relates several experiments upon different species of quadrupeds—two cats, a she-goat, a rabbit, a lamb, a he-goat, and a sheep—in which the *trachea* was opened, and various irritating substances, such as oil of vitriol, butter of antimony, and fumes of sulphur, were introduced into the air-passages, without exciting cough. Some of the animals gave indications of suffering, and breathed forcibly. The sheep was supposed to be phthisical. Every practical surgeon is aware of the little uneasiness produced by the introduction of a canula into an opening in the *trachea*. The violent paroxysms of coughing consequent upon the introduction of foreign bodies into the *trachea* from the mouth seem principally dependent upon their passage through the *larynx*. I have suggested that Brachet in some of his experiments may have overlooked a source of fallacy which would very seriously interfere with the results—and that is, the facility with which some of the irritants used might reach the interior of the *larynx;* for it is to be remembered that, in the experiments which he details to illustrate the annihilation of the sensibility of the inner surface of the *trachea* and bronchial tubes after

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[1] Opera Minora, tom. i., p. 402, Laus. 1762; or Sur la Nature Sensible et Irritable, tom. i., p. 394. Laus. 1756.

division of the *vagi*, the nerves were cut in the middle of the neck. It is quite possible that this may affect the results in some experiments more than in others, and it is evident that, if the head be depressed when a quantity of fluid is thrown into the *trachea*, or if the fumes from a muriatic acid bottle be used as an irritant (as by Brachet in some of his experiments,) the chances are, that a part of these will reach the interior of the *larynx* and excite violent efforts to cough.[1] In the last communication we illustrated at some length (p. 113) the great difference in the acuteness of sensibility between the mucous surface of the *larynx*, and that portion of the air-passage placed below it; and the same thing was remarked by Mr. Key in an operation on an individual of the human species for the extraction of a foreign body from the *trachea*.[2] I have performed several experiments with the view of satisfying myself regarding the effects of the division of the *vagi* upon the sensibility of the mucous membrane; and though I have seen good reason to believe that it is much blunted, I am by no means satisfied that it is entirely annihilated. I must also state that it is very difficult to procure a decisive experiment on this point. The animal frequently becomes restless and uneasy after section of the nerves, if care be not taken to secure a sufficient supply of air to the lungs; and it may make forcible expirations not unlike a cough when the mode of irritating the inner surface of the *trachea* is such as to interfere materially with the passage of the air along it. This, however, in all probability, may arise from the sense of anxiety occasioned by the impediment to the

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[1] Müller refers to the experiments of Krimer, as corroboratory of those by Brachet on this subject. (Baly's Translation, vol. i., p. 353.) As I have not yet been able to procure the treatise by Krimer in which these experiments are detailed, I am ignorant of the manner in which they were conducted.

[2] Lancet, 1828-29, vol. ii., p. 661.

respiration. Another source of error arises from the great facility with which the air-passages tolerate stimulants, which at first excite great uneasiness, as was well illustrated in the practice followed by Dessault.[1] I shall now detail a few of the experiments I made to illustrate these sources of fallacy I have mentioned, and also show that the sensibility of the mucous membrane of the *trachea* and *bronchi* is at least blunted after division of the *vagi*.

28*th Exper.*—The pneumogastrics were exposed without disturbing them in a large terrier dog. A small opening was then made into the *trachea*, and a quantity of water was thrown down towards the lungs (the head being kept erect) without exciting cough. A small quantity of alcohol was then injected downwards, and this was followed by efforts to cough. After these had ceased, it was thought advisable to witness their repetition. A fresh quantity of alcohol was therefore injected downwards, but the animal remained perfectly quiescent. This was again repeated with the same results. It was now apparent that it was useless to proceed farther, so that, instead of dividing the *vagi*, we injected half an ounce of air (by measure) along the carotid artery towards the brain. This was followed by strong convulsions of the limbs and trunk, succeeded by coma, which continued for at least four hours—the time it was last seen alive. It occasionally howled and moved its limbs rapidly.[2]

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[1] Oeuvres Chirurgicales par Bichat, tome ii., p. 266. 1801.

[2] I have repeated this experiment of blowing air along one of the carotids or vertebrals towards the brain, in animals which had not been previously subjected to the influence of any narcotic, and always found that it produced convulsions and coma, lasting for some time before death. The quantity of air injected was from half an ounce to an ounce, and even more. The mode of death in such cases is very different from what I have observed when air is injected *rapidly* and in *considerable* quantity into a vein leading directly to the heart, for it then kills by mechanically arresting the movements of the right side of the heart.

29*th Exper.*—The *vagi* were exposed in the middle of the neck, but not disturbed, in an ordinary sized terrier. A small opening was then made into the *trachea*, and some cold water injected downwards, the head being kept erect. This was followed by slight efforts at coughing. The nerves were then divided, and the animal became restless and uneasy. Both water and alcohol were injected downwards after division of the *vagi*, without exciting cough as long as the head was kept erect, while it occurred when the head was placed in a depending position.

The alcohol injected was at first in small quantities at a time, and after short intervals. The whole quantity injected was two drachms, and this produced some symptoms of narcotism.

30*th Exper.*—The *vagi* were exposed in the middle of the neck, but not disturbed, in a very lean and large mongrel dog, lately recovered from the distemper. A very small opening was made into the *trachea*, and a quantity of cold water thrown downwards without inducing cough. A small quantity of alcohol was then injected, and three or four distinct coughs followed. The *vagi* were then divided, and this was followed by no impediment to the respiration. Alcohol to the extent of half an ounce in all was thrown in divided quantities down the *trachea*, without exciting any efforts to cough. The animal was at last evidently affected by the alcohol, for when let loose it staggered in walking.

31*st Exper.*—The *vagi* were exposed without being disturbed in a young middle-sized mongrel. Water injected into a small opening in the *trachea* excited pretty urgent cough, and it appeared uneasy and coughed frequently when alcohol was injected. Section of the *vagi* was followed by struggles and symptoms of suffocation. It recovered from this apparently to a considerable extent; and some alcohol was then injected into the *trachea* without ex-

citing uneasiness or cough. A drop of muriatic acid was also placed in the *trachea* without exciting uneasiness. Some alcohol was now injected towards the *larynx*, and this was followed by uneasiness and cough. The animal was now killed by a dose of prussic acid. In none of these three last experiments was any cough excited by irritation of the inner surface of the *trachea* and *bronchi* after division of the *vagi*, but I could adduce two experiments where a large tube was first introduced into the *trachea* before the division of the *vagi*, in which uneasiness and forcible expirations, which I could not distinguish from an effort to cough, were excited, when irritating substances were injected into the *trachea*. The details of the 28th Experiment are very instructive. If we had proceeded to divide the *vagi* after the first injection of the alcohol without repeating it, we might have attributed the different results that followed entirely to the section of the nerves. We cannot assert that the same thing may not have occurred in one or more of the three other experiments. It is worthy of remark, that in four dogs which lived beyond the fourth day after section of the *vagi*—in three of which the recurrents were also divided—frequent cough, or at least forcible expirations, which I could not distinguish from a cough, were observed. The animal the subject of the 14th Experiment, coughed so incessantly during the three last days of its life, that I could not reckon the respirations until after many trials, and even then imperfectly. Whether this was dependent upon any irritation of the upper part of the cut nerves, or upon any irritation of the mucous surface of the lungs, or upon any other cause, I cannot pretend to determine. I may, however, mention, that the examination of the cut ends of the nerves after the death of these animals did not disclose any marks of high inflammation. In an experiment by Valsalva, where the *vagi* were cut in a young dog which lived to the

eighteenth day, the animal is described as coughing on the third and some of the subsequent days.[1]

Morbid changes in the Lungs.—I have removed a portion of both *vagi* in seventeen dogs, for the purpose of examining the morbid changes in the lungs consequent upon section of these nerves. In ten of these the recurrents were also divided. The time which twelve of them lived after division of the nerves is given in the tables illustrating the diminution of the frequency of the respirations after this operation. I may also add here the period of time the other five lived. In the 32d Experiment the animal lived thirty-one hours; in the 33d Experiment it lived thirty hours and a-half; in the 34th Experiment it was killed after ninety-six hours; in the 35th Experiment it lived fifty-five hours; and in the 36th Experiment it lived twenty-one hours. In all of these last experiments except the 35th, a tube was introduced into the *trachea.* We find on examining the period of time these seventeen animals lived, that four died before the completion of twenty-four hours, and in three of these a tube had been introduced into the *trachea;* three died between the twenty-fourth and forty-eighth hour—all of which had tubes introduced into the *trachea;* four died between the forty-eighth and seventy-second hour, none of which had tubes introduced into the *trachea;* one died between the seventy-second and eighty-sixth hour; in this animal no opening was made into the *trachea;* one was killed after four days and eight hours, and though an aged

[1] Valsalvae Opera cum Epistolis Anatomicis J. B. Morgagni, Epistol. Anatom. xiii. 36.—Supposing it to be proved that excitants applied to the mucous membrane of the lungs can excite sensation after section of the *vagi,* it would not necessarily follow that the impressions which give rise to these sensations are conveyed through the grey filaments proper to the ganglionic system, since it appears (Müller's Physiology, pp. 664-672) that a certain number of the white filaments of the cerebro-spinal system accompany all the branches of the ganglionic system.

dog, had an opening made into the *trachea;* two died after the completion of the eighth day; one died on the twelfth day; and one was killed after the completion of the twelfth day. In the last four experiments the animals breathed freely without opening the *trachea.* To these seventeen experiments I could also add several others, where the animals were killed at longer or shorter periods after division of the nerves, and to some of which I shall have to refer. The young dogs subjected to these experiments generally died earlier than the more aged, as may be inferred from the circumstance, that the greater number of those which lived longest did not require the introduction of a tube into the *trachea.* The lungs were in a state unfit for the healthy performance of their functions in fifteen out of the seventeen animals experimented upon. One of the two animals whose lungs were found healthy (10th Experiment) died after the completion of the eighth day, apparently from inanition; the other (13th Experiment) was killed after the twelfth day, when it was apparently in perfect health. More extensive experience has enabled me to make some material alterations upon the few observations I made on these points in my last communication, (p. 142.) The most common morbid changes found in the lungs of those animals was a congested state of the blood-vessels of the lungs, and the effusion of frothy serum into the air-cells and bronchial tubes. In eight out of the seventeen these appearances were strongly marked. In some portions of the lungs the quantity of blood was so great as to render them dense. The degree of congestion varied in different parts of the same lung, but it was generally greatest at the most depending portions. This condensation was not unfrequently greater than what could be accounted for by mere congestion of blood in the vessels, and probably arose from the escape of the solid parts of the blood into the tissue of the lung. The frothy serum had

frequently a more or less deep tinge of red. Some of those animals were opened immediately after death, which in three cases was hastened by a dose of prussic acid, given when the animal had become cold and stupid. In the 16th, 20th, 22d, and 32d Experiments, though different parts of the lungs were much loaded with blood, the quantity of frothy serum found in the bronchial tubes was but trifling, and certainly not sufficient to impede the respirations to any great extent. In the 16th Experiment the animal lived fifty-two hours; and it is stated in my notes of the dissection, that there was a minute quantity of frothy serum in the larger bronchial tubes, and a small quantity of serum could be pressed from the minute bronchial tubes. A great portion of both lungs was so dense as not to crepitate when cut, was of a dark colour, and sank in water, though the cut surfaces were smooth, and did not present the granulated appearance of the second stage of ordinary pneumonia. In the 20th Experiment, the animal died between the seventy-sixth and eighty-third hour. Without giving the details of the dissection as they are written out in my notes, I may merely mention that part of the left lung was in a state of gangrene, and that some portions of the right lung were dark-coloured and dense, but floated in water. In the 22d Experiment, the animal was killed after sixty-nine hours, and after it had become feeble. The lungs were dense, and there was only a small quantity of frothy serum in the bronchial tubes. In the 32d Experiment, the animal lived thirty-one hours. The chest was opened immediately after death, in fact as soon as it had ceased to breathe. The lungs were dense and loaded with blood, but there was very little frothy serum in the larger and smaller bronchial tubes. In the 11th Experiment the animal received a dose of prussic acid on the eighth day, after it was evident, from the state of its breathing, that it could not survive, and the chest was immediately opened. A great part of the left lung was in a state of

pneumonia, in some places approaching the third stage, and many of the smaller bronchial tubes were full of a puriform matter. The right lung was dark-coloured and rather dense in some parts, but these when cut out floated in water. There was no appreciable quantity of frothy serum in the bronchial tubes of the right lung. In the 15th Experiment the animal died on the twelfth day. The left lung was voluminous, of a deep dark-colour, and dense. When cut into, little blood could be squeezed out, and the cut surfaces were not granular. The right lung was somewhat congested with blood at a few parts. The *trachea* contained some, and the left bronchial tubes much red mucus; while the bronchial tubes of the right lung were empty. In the 34th Experiment, the animal was killed after four days and eight hours. The right lung was nearly healthy, while the left lung was almost entirely in the second and third stages of pneumonia, and there was very little serum in the bronchial tubes. Of the eight experiments in which the frothy serum was effused in great quantity into the bronchial tubes, the left lung was partly gangrenous in one, (21st Experiment.) In other two experiments, 35th and 36th, part of the lungs was in the second and third stages of pneumonia, with effusion of puriform matter and much frothy serum into the bronchial tubes. In the seventeen experiments, therefore, five presented distinct traces of pneumonia, and in two it had run on to gangrene. In one of these the gangrenous portion was surrounded by circumscribed abscesses. In these experiments, I need scarcely again remark, great care was taken to secure a free passage to the air along the *trachea*, and this was apparently accomplished, except in the 33d and 36th Experiments, where the tracheotomy tubes employed were too small.

One of the most important points to ascertain, in an investigation of this kind, is the first departure from the

healthy state; to decide whether the effusion of the frothy reddish serum, by interfering with the usual changes of blood in the lungs, *causes* the congested state of the pulmonary blood-vessels and the laboured respiration; or whether this effusion is the *effect* of a previously congested state of the blood-vessels. If it be made out that the effusion of serum is consequent upon the congested state of the blood-vessels, we have next to inquire—what is the probable cause of this congested state of the pulmonary blood-vessels? In examining, with a reference to this question, the results of the experiments we have made, one of the most important circumstances that presents itself is the fact, that the effusion of frothy serum into the bronchial tubes, in quantities sufficient to impede materially the respiration, is not a necessary consequence of division of the *vagi*, even when the lungs were found loaded with blood and when the respiration before death was very laboured. And this naturally leads us to doubt whether the frothy serum is the cause of the laboured respiration and the congested state of the pulmonary blood-vessels in those cases where it is present, though there can be no doubt that, when once it is effused, it must powerfully tend to increase the difficulty of the respiration and the impeded circulation through the lungs. Another important circumstance in enabling us to decide this interesting point is derived from the fact, of which I have satisfied myself after much careful examination, that this frothy fluid is not mucous, though it is occasionally mixed with it, but is the frothy serum so frequently found in cases where the circulation through the lungs has been impeded for some time before death. To investigate this point still farther, we killed several dogs at longer and shorter periods after section of the *vagi*, and we were more and more confirmed in the opinion, that the congestion of the blood-vessels is the first departure from the healthy state of the lungs, and that the effusion of frothy serum is

a subsequent effect. Perhaps one of the most illustrative experiments I can select is the following:—

37*th Exper.*—The recurrents and *vagi* were cut across and a portion removed in a middle-sized bull-dog at two o'clock, P.M. At seven, P.M., his breathing was easy, and the respirations were eight in the minute. Next day at two, P.M., the respirations varied from ten to twelve. On the following day the respirations were six to seven, easy but heaving. On the third day at eleven, A.M., the respirations were still between six and seven in the minute, very heaving and prolonged, and the animal was evidently becoming very feeble. The femoral artery was exposed and a small branch opened, when the blood escaped in a feeble stream, and was almost as dark-coloured as venous blood. A fatal dose of prussic acid was now given, and the chest immediately laid open, care being taken to avoid the large vessels at the root of the neck. The large pulmonary vessels were now opened, and the blood both in the arteries and veins was dark and fluid. The lungs were not emphysematous, and contained much blood in their vessels. The left lung was of a deep dark colour at the summit of the anterior lobe, and this when cut into was found quite dense, without granules, and scarcely any blood could be pressed from it. The bronchial tubes of this part contained some puriform mucus. A similar but smaller dense portion was situated about the middle of the same lung. Slices from these two portions sank in water. The right lung contained much blood; some parts of it were denser than others, but none of it sank in water. The mucous membrane of the bronchial tubes was moist and lined by a thin layer of mucus. A small quantity of frothy serum was found in some of the bronchial tubes.

If the congested state of the blood-vessels precede, as we believe, the effusion of the frothy serum, we have next to inquire, what is the cause of the retardation of the blood,

and congestion of the blood-vessels in the lungs? This, we were formerly inclined to believe, might depend upon paralysis of the muscular fibres of the bronchial tubes; but, being unable to obtain any satisfactory evidence of this, we again watched the phenomena more narrowly, and now believe that all the morbid changes observed in the lungs can be traced to the diminished frequency of the respiratory muscular movements. We have already dwelt at some length upon the influence of the *vagi* as exciters of respiration; and we have shown that when these nerves are tied or divided, the number of the respiratory muscular movements is instantly considerably lowered—generally more than one-half. Now, it is an established fact, that the flow of blood through the lungs is dependent upon the continuance of the respiratory process, and the great diminution in the activity of the respiratory muscular movements must be followed by a retardation and congestion of the blood in the lungs. This congestion of blood, as is well known, is generally followed by effusion of serum, and also predisposes the organs so circumstanced to various morbid changes, chiefly of an inflammatory kind. In the lungs this congestion is not only followed by the escape of the serum from the vessels, but also of the more solid materials, rendering the tissue dense. The effused serum is mixed up with the air moving along the bronchial tubes during inspiration and expiration, and it thus becomes frothy. A little blood also exudes from the congested mucous membrane, giving the serum a reddish tinge. Such, we believe, is the explanation of the morbid changes observed in the lungs after lesion of the *vagi*. If artificial respiration, which is a very imperfect substitute for the natural process, be carried on for some time in healthy lungs, a similar effusion takes place. Legallois, who must have been perfectly familiar with the appearance of this effused fluid, after stating that its formation was accel-

erated by artificial respiration in animals after section of the *vagi*, and after decapitation, is obliged to admit that a similar fluid is very frequently formed in animals not subjected to either operation when artificial respiration is carried on. "Car très-souvent il s'en forme un semblable dans les animaux entiers qu'on insufle."[1] We would not wish peremptorily to deny that the effects of the diminished frequency of the respiration may not be aided by paralysis of the muscular fibres of the bronchial tubes; we only state that after the most anxious endeavour to obtain some distinct evidence of this, we have been unsuccessful. Some may be inclined to believe that the *par vagum* may exert some favourable influence upon the capillary circulation of the lungs. The thing is possible, but we know of no well-ascertained facts which could be adduced even as an analogical argument in favour of such an opinion.

I have not unfrequently observed in the human species, where the respiration had been impeded for some time before death, a condition of the lungs similar to that observed in the lower animals after division of the *vagi*. In several of the fever patients I have had lately occasion to inspect at the Royal Infirmary, the lungs in the posterior and middle parts were dark-coloured and gorged with blood and serum. When cut into, the substance of the lungs in some cases appeared at different parts denser than what could be accounted for by mere congestion of the blood-vessels, and a comparatively small quantity of blood could be squeezed from the cut surfaces, though the blood in other parts of the body was fluid; but sections of the lung generally, though not always, floated in water, and presented none of the granular appearance. In some cases the bronchial tubes contained a considerable quantity of

[1] Expériences sur le Principe de la Vie, p. 242. 1812.

frothy serum; in others very little. In all probability these morbid appearances are occasionally dependent upon the disturbed respiration consequent upon derangement of the central organs of the nervous system. I lately saw a gentleman labouring under fever, whose respirations for a short time were only eight in the minute, though the lungs at the time were unaffected, but they fortunately soon rose to sixteen in the minute, and he ultimately recovered. Dr. Alison has suggested to me that these morbid changes in the lungs are sometimes owing to another cause. He believes that, in cases of fever, where the heart's action is feeble, the bronchiæ often somewhat obstructed, and the blood altered, the right side of the heart is unable to propel the blood through the lungs; it consequently goes on accumulating in their depending parts, and the same results follow as when the respiratory movements are diminished in frequency. In confirmation of this view he stated that he has seen these morbid changes occur in the lungs without any preceding diminution of the respiration. When we remember that the pulmonic circulation is dependent upon two distinct causes varying in efficacy—the contractions of the right side of the heart, and the chemical changes going on at the lungs—both of which are necessary for the proper propulsion of the blood through the lungs to the left side of the heart, we can easily understand how a diminution in the activity of the respiratory muscular movements, and the impaired contractility of the right side of the heart, should produce the same effect, viz., congestion of blood in the lungs and effusion of frothy serum.[1] I lately examined the bodies of two patients who

[1] The fluid state of the blood in fever will naturally aid in producing this congested state of the pulmonary vessels. *Vide* Magendie's Leçons sur les Phénomènes Physiques de la Vie, tom. iv. 1838. As far as my observation goes, the coagulability of the blood is not materially affected after section of the *vagi*.

died in the Royal Infirmary, which afforded a very important confirmation of the views we have adopted regarding the cause of the morbid changes in the lungs after lesion of the *vagi.*

CASE I.—James Hallam, a sailor, aged 38, was admitted into the Royal Infirmary on the 30th November last, with a slight attack of *bronchitis*, from which he rapidly recovered under the usual remedies. On the 6th December it was reported that he had no complaint. On the 11th it was remarked that he had been drowsy and almost constantly asleep since the day preceding, but he was easily roused, answered questions, and made no complaint. The pupils were contracted and little sensible to light. The respirations were only five in the minute, and heaving. Some appearance of *anasarca*. Urine spec. grav. 1009, coagulable by heat and nitric acid; pulse of moderate strength. On the 12th the respirations were still only about five in the minute, heaving and prolonged, and exactly similar to what are observed in dogs after section of the *vagi.* On the 13th the respirations varied from five to ten, and were still heaving and prolonged. On the morning of the 14th the respiration remained of the same character. He had passed no urine since yesterday, and three ounces were drawn off by the catheter. He died at noon in slight convulsions.

Sectio cadaveris, 16*th.*—The arachnoid membrane appeared somewhat dry, and the convolutions were perhaps rather flattened. The vessels on the surface of the brain were well filled with blood, and a considerable number of red points presented themselves on slicing the brain. The substance of the brain was not firmer than natural, and there was little fluid in the ventricles. The heart was healthy, and the right side was filled with coagulated blood. There was a little reddish serum in the right side of chest. Lungs emphysematous along their anterior margins. The bronchial tubes were full of frothy serum, and the whole of

the posterior and middle portions of both lungs were of a dark, red colour, and were full of blood and serum. Some parts of the lungs were so dense as not to crepitate when cut, but they presented none of the granulated appearance. The liver was natural, with the exception of the presence of an old cicatrix. The kidneys were of a yellowish colour externally, and their capsule was firmly adherent. The cortical structure of the kidney was nowhere apparent, and its place was occupied by a yellowish substance, which also encroached in some places upon the tubular structure. The kidneys were of their usual size, and firmer than natural. The trunks of the *vagi* were examined at their origin, at the middle and lower part of the neck, and within the thorax, and no morbid appearances observed.

Case II.—Peter Ballantyne, aged forty, a tailor, was admitted into the Royal Infirmary on the 14th December, with cough and expectoration tinged with blood. He also complained of palpitation, which was much increased by exertion. He stated that he had been subject to headache and vomiting, and his friends mentioned that he had been lately languid and indulged in long sleeps. When admitted the respirations were fourteen in the minute. Urine scanty, specific gravity 1011, coagulable by heat and nitric acid. The pupils were contracted, and he appeared to have a tendency to drowsiness. After his admission the cough and expectoration diminished, but the strong action of the heart continued, and the urine retained its former characters. On the 18th the cough was slight; his respirations had fallen to five in the minute; pulse 96, small; countenance exsanguine; was very drowsy, but answered questions. On the 19th, he gradually verged into a state of coma. The respirations varied from five to eight in the minute. The pupils were immovable and contracted. Pulse 84, rather feeble, but extremities warm. He had passed no urine for several hours. There was no œdema.

No paralysis was observed during life. He died at six o'clock, P.M.

Sectio cadaveris, 21*st*.—The surface of the brain was unusually pale; few red points presented themselves on slicing the brain, and the vessels appeared to be principally filled with serum. There were about two drachms of serum in the lateral ventricles, and half an ounce at the base of the brain. Two small clots of blood were found in the right *corpus striatum*, without surrounding softening. In the central portion of the anterior part of the *tuber annulare* there was a small cyst lined with a yellow membrane; and immediately posterior to this there was a small clot of blood about the size of a large pin's head. The large blood-vessels of the brain were dilated and thickened in their coats, but without calcareous deposit. The trachea and bronchial tubes contained a considerable quantity of frothy serum. The posterior and middle parts of the lungs were gorged with blood and frothy serum, and a considerable portion was so dense as not to crepitate when cut; but it did not present the granulated appearance, and floated in water. There was a mass of old tubercles in the apices of both lungs. The left ventricle of the heart was considerably thickened in its walls without diminution of its cavity. The valves were quite sufficient to fulfil their functions, and the thoracic aorta retained its elasticity. The kidneys were rather smaller than usual, and had numerous yellowish spots scattered through the cortical portion. The *vagi* were examined at their origin, at the lower part of the neck, and within the thorax, and no morbid appearance could be detected.

We were prevented by the friends in this, as in the former case, from examining the *vagi* through their whole extent; but this is perhaps a matter of little importance, as it was sufficiently evident from the symptoms that the drowsiness and slow respiration in both arose from cerebral

derangement. In the first case, this in all probability arose from the accumulation of urea in the blood, exerting a deleterious influence upon the brain—a not unfrequent concomitant of kidney disease.[1] It may be a matter of doubt whether the cerebral derangement in the second case arose from the same cause, or from the effusion of blood. The symptoms certainly presented themselves in a more gradual manner than we should expect in cases of apoplexy, and more resembled the coma which occurs in kidney disease. I may state that Dr. Christison has seen cases of coma in kidney disease where no œdema was present.

We have embraced every opportunity of examining the effects of lesion of the *vagi* upon the secretion of mucus from the inner surface of the bronchial tubes. We have already stated that we believe the frothy fluid so frequently found in the bronchial tubes is not mucus and the result of a morbid secretion, as Wilson Philip[2] and others have imagined, but that it is chiefly, and in some cases almost entirely, composed of serum, and is consequent upon the congested state of the blood-vessels—an effect so frequently observed in other parts of the body. In killing animals at very various periods after section of the *vagi*, we have never found the inner membrane of the bronchi unusually dry, but always covered with the usual quantity of protecting mucus, except in those cases where inflammation was present. We can explain how the application of galvanism in the experiments of Dr. W. Philip was so efficacious in preventing this effusion, without being obliged to admit that the secretion of the mucous membrane is deranged; for, since the galvanism was applied so as to

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[1] *Vide* Christison on Granular Degeneration of the Kidneys. Edinburgh, 1838, p. 92.

[2] Inquiry into the Vital Functions, Chapters v. and xii.

keep up a constant spasmodic action of the limbs, the frequent contractions of the respiratory muscles produced by this excitation would prevent the occurrence of the diminished frequency of the respiratory movements. The explanation given by Brachet[1] of the source of this frothy fluid—viz. that it is the usual mucous secretion of the lining membrane of the air-passages which has accumulated there, because this membrane has lost its sensibility, and the muscular fibres of the bronchi have lost their contractility —is so palpably objectionable as to require no remarks.

An excellent illustration of the numerous difficulties with which the physiologist has to contend, from the impossibility of insulating any individual organ from its mutual actions and reactions with others, when he wishes to examine the order and dependence of its phenomena, is furnished by the experimental history of the *par vagum*. The two first experimenters, Rufus of Ephesus,[2] and Galen,[3] attended only to the loss of voice. A new direction was then given to this investigation by the suggestion of Piccolhomini[4] that it affected the heart's action, and numerous experiments were afterwards adduced in support of this opinion. Some described its effects as instantly fatal. F. Schrœder stated, that when both nerves are tied, the animal dies instantly—"animal vitam protinus amittit."[5] Bohnius, in mentioning an experiment of this kind, indulges in a highly figurative expression, which appears to be still a favourite with some physiologists, and describes the animal dying as if struck with a thunderbolt, "fulmine quasi tactum."[6] Varignon relates, that when he

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[1] Oper. cit., p. 160.
[2] *Vide* Morgagni de Sedibus et Causis Morborum, Epist. xix. Art. 23.
[3] De Locis Affectis, lib. i., cap. 6.
[4] Anatomicæ Prælectiones Arch. Piccolhomini. Romæ, 1586, p. 272.
[5] Addit. ad Vesling. Synt. c. 10, n. 7, as quoted by Morgagni.
[6] Circul. Anatom. Progym. vi., p. 104. Lips. 1686.

had tied these nerves in a cat, the animal "était mort dans l'instant sans aucun mouvement d'aucune partie de son corps."[1] Some experimenters, however, such as Willis, Vieussens, and many others, found that the operation was not always instantly fatal, and that the animal might live twenty-four hours, and even several days: and though puzzled to account for this on their favourite theory—that a constant supply of animal spirits flowed along the vagus to the heart—yet they succeeded in accommodating the facts to this explanation, by supposing that a diminished supply of the vital spirits continued to reach the heart through the more circuitous and less favourable channel of anastomoses.[2] Little progress was made in this inquiry for a long series of years, and the prevailing doctrine appears to have been, that section of the nerves, some way or another, affected the heart's action. The experiments of Dupuytren[3] are chiefly important, as they directed the attention of physiologists in an especial manner to the derangement of the lungs. No doubt the derangement of the respiratory function had been previously remarked by Valsalva, Morgagni, Molinelli, Haller, Brunn, Cruickshank, and some others. Valsalva states, however, that nothing morbid was observed in the chest of a dog he dissected after death from this operation.[4] Morgagni found the lungs congested with blood.[5] Mr. Cruickshank, in 2d Experiment, where the animal died on the seventh day after division of the second *vagus*, ascertained that every organ was healthy except the lungs, and these he describes to

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[1] Histoire de l'Acad. Roy., p. 28. 1706.

[2] Willis Cerebri Anatome, caput xxiv., 1665; Vieussens Neurographia Univer., liber iii., caput iv., pp. 179, 180. Lugduni, 1716.

[3] Biblioth. Med., tom. xvii., p. 1, 1807. An abridgment of this memoir is given in the Journal Médic. de Corvisart., tom. xiv., p. 45.

[4] Valsalvæ Opera, &c., Epist. Anatom. xiii. 36.

[5] Oper. cit. Epistol. Anat. xiii. 29.

have been of a red-brown colour, so dense as to sink in water, and containing frothy fluid with some puriform matter in the bronchial tubes. In the 3d Experiment, he found the lungs loaded with blood.[1] Dupuytren concluded from his experiments—and in this Hallé and Pinel, the reporters on the memoir, concurred—that animals after lesion of the *vagi* die of asphyxia, because the atmospheric air, although continuing to penetrate freely into the lungs, can no longer combine with the blood, since this combination cannot take place except under the agency of the nervous system. This is also the opinion which Sir Astley Cooper seems disposed to adopt in the account of his experiments on the *vagus*, given in Guy's Hospital Reports, already referred to.[2] Soon after the publication of Dupuytren's memoir, it was shown by the experiments of Dumas and De Blainville in France, and of Emmert in Germany, that arterial blood may continue to circulate in the arteries after section of the *vagi*. Dumas proved that the arterial colour could be restored by artificial inflation of the lungs after the blood in the arteries had become venous.[3] Emmert satisfied himself that, after the trachea had been compressed in rabbits and cats whose *vagi* had been divided, the arterial hue of the blood could be restored by removing the pressure, and allowing the air again to enter the lungs.[4] It was likewise proved by Sir B. Brodie in this country, that the usual chemical changes take place at the lungs in a decapitated animal when artificial respiration is kept up.[5] Provençal, in France, also showed that an animal continues to deteriorate the air by the formation of carbonic acid

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[1] Medical Facts and Observations, vol. vii.

[2] Opus cit., p. 470.

[3] Journal Général de Médecine, tom. xxxiii., p. 353.

[4] Archiv für die Physiologie, von den Professoren Reil und Autenrieth. Neunter Band. S. 380, 1809, und Eilfter Band S. 117, 1812.

[5] Philosophical Transactions, 1811, p. 36.

gas, after the *vagi* have been divided, though not to the same extent as before the operation.[1] The experiments of Legallois[2] at last came, and satisfactorily explained all the anomalies connected with the occasionally rapid death of the animal, and the sudden arrestment of the chemical changes at the lungs, which had so long puzzled physiologists. He pointed out by decisive experiments, that when the *vagi* are divided, more especially in young animals, the superior aperture of the larynx frequently becomes closed, and the animal is suffocated. He also fully described the congested state of the blood-vessels of the lungs and the effusion of frothy fluid. Legallois, however, seems to have imagined that the effused frothy fluid was the cause of all the dyspnœa, and that this effusion in its turn was dependent upon a kind of paralysis in the lungs. The same morbid appearances in the lungs pointed out by Legallois have been frequently observed and described by more recent experimenters, among whom we may mention Dr. Wilson Philip,[3] Dr. Holland,[4] and M. Brachet.[5] In examining the morbid appearances in the lungs, we have observed no facts in confirmation of the statement of Mr. Swan, that section of the vagi produces emphysema of the lungs.[6] We have undoubtedly seen portions of the lungs emphysematous in a few cases, but we had no reason to believe that this was the necessary result of the section of the nerves. That the lungs should collapse imperfectly when gorged with blood and frothy serum, is certainly no proof that they are emphysematous. Neither have we seen any thing confirmatory of the opinion of Mayer,[7] that if

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[1] Bulletin des Sciences Médic., tom. v., p. 361.

[2] Sur le Principe de la Vie, 1812.

[3] Oper. cit.

[4] An Experimental Inquiry, &c. Edinburgh, 1829.

[5] Oper. cit.

[6] Essay on the Connexion of the Heart and Arteries, &c., pp. 121-126. 1829.

[7] Tiedemann's Zeitschrift für Physiologie, Zweiter Band, S. 74-5, 1826.

the animal live more than forty-eight hours, the arteries and veins of the lungs, even to their minute ramifications, as well as the cavities of the heart, are filled with white, firm, and compact coagula, consisting of the fibrine and albumen of the blood; while if the animal die sooner, the coagula are soft and dark-coloured. It is these coagula, according to Mayer, which cause the arrestment of the movements of the heart and produce death. He believes that when the influx of the nervous influence is arrested by division of the vagi, the fluidity of the blood ceases, and it separates itself into its constituent parts, as when drawn from the body. Müller has been equally unsuccessful in finding these fibrinous clots in the pulmonary vessels.[1]

The explanation of the fact, that lesion of one *vagus* does not necessarily nor even generally induce disease of the lung of that side, may, I believe, be satisfactorily obtained, when we consider that this is not likely to diminish the number of respirations so much as the lesion of both *vagi*. We have in several dogs cut the *vagus* of one side, and after having observed the changes produced we then divided the *vagus* of the opposite side. Some of the results will be found in the following table:—

Respirations before Operation.	One Vagus Divided.	Both Vagi Divided.
Exp. 1st, 14 to 16	11 to 12	5 to 6
— 2d, 15	13	6 to 7
— 3d, 16 to 17	9 to 10	6 to 7
— 4th, 16 to 18	12	8

In other two experiments the frequency of the respirations was not sensibly affected by section of one *vagus*, though the animals were kept for eight days. In one of these the other *vagus* was cut, and the frequency of the respirations suffered a very marked change, falling from twenty to eight in the minute.

[1] Elements of Physiology, vol. i. p. 358. Baly's translation, 1838.

In examining the effects of lesion of the *vagi* upon the lungs, it is particularly worthy of remark, that these morbid changes in the lungs do not necessarily follow the division of both *vagi* and recurrents. In a dog, Experiment 13th, which was killed in presence of Dr. Alison, twelve days after the section of the *vagi* and recurrents, the lungs were found perfectly healthy, though the respirations before death were still slow and heaving. This animal had recovered from the effects of the operation on the digestive system, and was rapidly gaining flesh and strength. In dissecting the nerves after death, the cut ends of the left *vagus* were one inch and one line apart; and the distance between the cut ends of the right was one inch and two lines. In another dog, Experiment 10th, which died apparently from inanition eight days after division of the *vagi* and recurrents, the lungs were also found healthy. The nerves in both of these animals were carefully examined after death, and no anormal arrangement of them could be observed. Previous to the performance of Experiment 13th, I fully believed in the correctness of the opinion expressed by Magendie[1] and Müller,[2] that the simultaneous division of both *vagi* is always fatal within a few days. I have, however, no doubt in my own mind, and this was the opinion of all who saw this animal after the tenth day, that he had fairly recovered from the operation. About the sixth and seventh days after the operation he was so weak as to stagger occasionally in walking, while at the time of his death he was so lively and active as to leap over places between three and four feet high for his amusement. I afterwards regretted that I did not permit him to live longer, but I then had hopes that two others would survive the operation. In this I was, however, disappointed. I

[1] Milligan's Translation, p. 398. 1831.
[2] Baly's Translation, vol. i., p. 356.

find that M. Sédillot, in detailing an experiment, (to which I shall have occasion again to refer,) where the animal lived two months and a-half after section of both *vagi* with loss of substance, states that there was no induration of the lungs, and that they contained little blood. In examining the details of the very numerous experiments that have been made upon the effects of the division of both *vagi*, we shall find that by far the greater majority of animals die before the completion of the third day. In the seventeen experiments we have so frequently referred to, eleven died before the completion of the third day, and seven of the eleven before the completion of the second day. Valsalva remarks that animals live longer when the *vagi* have been simply divided than when they have been tied.[1] In one of Petit's experiments the animal lived seven days.[2] One dog experimented on by Baglivi died on the twelfth day, and another on the seventh day.[3] Morgagni details, in his additions to the works of Valsalva, the results of two experiments on dogs—one of which lived to the tenth and the other to the eighteenth day.[4] The eighteenth day is apparently the longest period he had seen an animal live after section of the *vagi*, as he refers to it with this view when speaking of the duration of life after this operation. None of the animals experimented on by Baglivi and Morgagni ever recovered from the operation, but gradually became weaker. Besides, we are led to believe that the nerves were simply cut across without loss of substance. M. Dupuy, in his experiments, found that horses lived to the fifth, sixth, and seventh day, when care was taken to admit a sufficient quantity of air into the *trachea*.[5] De Blain-

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[1] Oper. cit., Epist. Anatom. xiii., 28 and 38.

[2] Mémoires de l'Acad. Roy. A. 1727, Expér. 2, p. 6.

[3] Opera omnia, pp. 676, 677, Exper. 7 and 8. Antvverpiæ. 1715.

[4] Oper. cit., Epist. Anat. xiii., 36 and 37.

[5] Journal de Médecine, Chirurgie, &c., tom. xxxvii., p. 356. Décembre 1816.

ville, in giving the results of his experiments upon pigeons, informs us, that these animals died on the sixth or seventh day.[1] Legallois cut both vagi in thirty-six rabbits, from the age of one to forty days, and they all died between six and a-half and eighteen and a-half hours after. Sédillot states that, in one experiment, a dog lived twenty-one days; and that another dog lived some days more than two months and a-half after the operation, though more than an inch of the nerve was removed on both sides.[2] Arnemann has detailed an experiment at considerable length, where the animal appears to have completely recovered from the effects of the division of both *vagi* with loss of substance.[3] A portion measuring rather more than four lines was removed from the right, and one of eight lines from the left *vagus*. The animal at first suffered very severely from the operation, and it is described as gasping for breath. After a week it began to improve gradually, though the breathing was still difficult. After a month and a-half it had wonderfully improved, ate voraciously, but did not become fat. From the description given by Arnemann of the condition of the nerves in this animal when dissected after death, and from an engraving of the right *vagus*, (Tab. III. Fig. XVIII.) from which a smaller portion was removed than from the left, it appears that there was no regeneration of the removed portions of the nerves. The preparation of the right *vagus* was in the

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[1] Nouv. Bullet. de la Soc. Philom., tom. i., an. 2, p. 226.

[2] Thèse du Nerf Pneumogastrique, &c., Expér. 1 and 6, Paris, 1829. M. Sédillot also states, (p. 23,) that M. Begin kept a dog alive for a month after division of both *vagi*. I find that M. Chaumet states (Essai sur la Physiologie de l'Estomac, p. 17, Paris, 1828) that this animal operated on by Begin lived thirty-three days, and that no very obvious change was observed in the digestive process. M. Chaumet farther adds, that, in his own experiments, a dog lived fourteen days, and digested.

[3] Versuche über die Regeneration der Nerven.—Hundert und zehnter Versuch. S. 99, Göttin. 1787.

possession of Blumenbach. Notwithstanding the minuteness of the details of this case, and the apparent care with which the experiment was performed, and the nerves examined after death, we find that some authors express great suspicion of its accuracy.[1] It would appear that Arnemann had seen several dogs recover completely after division of both *vagi*, and that the animals were afterwards employed in other experiments upon the regeneration of some of the large nerves of the limbs; but I must confess that the statement is somewhat startling.[2] M. Brachet states in a note, that M. Fourcade had in 1820 presented to the Académie de Médecine, a dog in which four lines had been removed from the left *vagus*, and the same operation had been repeated on the right a few days after.[3] On this case M. Brachet remarks that, supposing the nerves to have been accurately divided, the free anastomoses between the laryngeal nerves at the *larynx* may have sufficed to transmit the nervous influence. In our experiment the recurrents were also divided.

Gastric Branches of the Vagus.

Are the gastric branches of the vagus partly nerves of motion? In my former communication (p. 121) it was stated that I had repeatedly seen muscular contractions of

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[1] Emmert. Oper. cit. Neunter Band S. 420. Lund, Physiologische Resultate der Vivisectionen neuerer Zeit. S. 241. Kopenhagen. 1825.

[2] Ich habe *mehreren* starken Hunden *beide Vagos* und einen Intercostalnerven zu gleicher Zeit durchschnitten. Nach einem *Monate* durchschnitt ich eben diesen Thieren mehrere grosse Nerven beider *Vorderbeine* und in der *Folge* noch beide *Ischiadische Nerven*. Von *allen diesen Thieren* habe ich kein einziges verloren, Oper. cit., p. 193.

[3] Oper. cit., p. 167. This experiment is also referred to by M. Sédillot, Oper. cit., p. 23. Neither Brachet nor Sédillot inform us how long this animal had lived, but Sédillot, after having given the details of his own experiment, when the animal lived somewhat more than two months and a half, adds, that this animal lived longer after this operation than in any other similar case on record, with the exception of that of M. Fourcade.

the œsophagus induced by irritating the trunk of the nervus vagus in the neck, extend also over the cardiac extremity of the stomach. In the stomach they were evidently slower, more prolonged, and vermicular than in the œsophagus, and they extended somewhat slowly from the cardiac orifice over a greater or less extent of the left portion of the stomach. Irritation of the vagus frequently fails to produce these movements in the stomach, even where the muscular fibres of the œsophagus are thrown into vigorous contractions. These muscular movements of the stomach from irritation of the vagus, have been frequently observed by Tiedemann and Gmelin.[1] They have also been inferred by Breschet and Milne-Edwards from the effects upon the digestive process of galvanizing the lower ends of the cut *vagi* in the neck of a living animal.[2] Mr. Mayo, however, could not perceive any muscular movements of the stomach upon irritating this nerve;[3] and the same thing is very strongly stated by Müller.[4] I have again carefully and repeatedly performed this experiment, and I am confident that though it frequently fails, yet it often succeeds. These muscular movements are, as I have already stated, different in their character from those of the œsophagus, and resemble more those of the intestines. I have been unable to satisfy myself whether the muscular movements seen on irritating the vagus are dependent upon any direct excitation of the muscular fibres of the stomach through the filaments of the vagus distributed upon it; or whether they are dependent upon the movements of the muscular fibres of the

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[1] Recherches Expérimentales Physiol. et Chim. sur la Digestion, &c. traduites par Jourdan, p. 374. Première Partie, 1826.

[2] Archiv. Gén. de Méd., tom. vii.

[3] Anatomical and Physiological Commentaries, No. ii. p. 15.

[4] Elements of Physiology, translated by Baly, p. 549.

lower part of the œsophagus being propagated onwards in the same manner as we find that contractions excited in the muscular fibres of the intestinal tube are propagated along it to a greater or less extent, and apparently without the aid of nerves. The supposition that the movements of the stomach resulting from irritation of the vagus are merely propagated onwards from the œsophagus, is favoured by the circumstances,—that the stomach frequently remains quiescent when the œsophagus is thrown into vigorous contractions; and that when these movements of the stomach do occur, they commence at the cardiac orifice, and extend themselves in a vermicular manner over the left extremity of that organ. I have likewise seen similar movements of the stomach produced by throwing the lower part of the œsophagus into contraction by pinching it with the forceps; but it is, however, quite possible that some of the gastric branches of the vagus were also included between the blades of the forceps. With the view of elucidating this point, I performed the following experiments upon a dog and three rabbits. In these animals the vagi were exposed in the neck, but without injuring them, from two to five hours after a full meal, and the stomach was then exposed by cutting through the parietes of the abdomen. I intended to have cut the *vagi* after watching the movements of the stomach, and to have observed the effects of the section upon those movements. The muscular movements of the stomach were however in all of these animals so indistinctly marked that I could obtain no satisfactory results; and, as the experiment was a cruel one, I did not persevere in it. That muscular movements may occur in the stomach after section of the *vagi*, sufficient to propel the chyme onwards into the *duodenum* is, we conceive, sufficiently proved by the experiments which we shall immediately have to examine in detail, where the lacteals were found full of chyle after section of both pneumogastrics in the neck. It

is the opinion of Breschet and Milne-Edwards,[1] and of Brachet,[2] that lesion of the vagi interferes with the proper performance of the digestive functions, by arresting the movements of the muscular fibres of the stomach; in consequence of which the aliments always preserve the same relation, and cannot be transformed into chyme except at the surface, or that part in contact with the walls of the stomach. Tiedemann and Gmelin also believe that the movements of the stomach principally depend upon the influence of the vagi.[3] That the gastric branches of the vagus are not partly motor we will not deny, but of this we have not been able to obtain any decided evidence. It, however, cannot be to the extent maintained by the authors we have mentioned, from facts already stated.

Effects of Lesion of the Vagi upon the Functions of Digestion.—That lesion of the *vagi* is generally followed by vomiting, (in those animals susceptible of it,) loathing of food, and arrestment of the digestive process, has been incontrovertibly proved by numerous experimenters. That perfect digestion may occasionally take place after division of the vagi in the neck, even when the cut ends of the nerves are kept apart from each other, we are also fully convinced. In four of the seventeen animals experimented on for the purpose of examining the morbid changes in the lungs, we obtained sufficient evidence of the restoration of the digestive process. The most satisfactory of these was the subject of Experiment 13th, to which we have already more than once referred. The animal operated on was a pretty large, full-grown, and lean bull terrier. Both recurrents and vagi were divided, and a portion of each removed. Immediately after the operation the respirations were much diminished in frequency, and continued so until its death,

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[1] Oper. cit., tom. vii., p. 197. [2] Oper. cit., pp. 204, 205.
[3] Oper. cit., p. 374.

but were quite easy, unless when the animal was made to struggle. The nerves were cut at half-past eight o'clock, A. M., on the 15th June, and at three, P. M., of the same day he ate some bread and milk, which he soon after vomited. He appeared to suffer no uneasiness, but was dull and languid. On the afternoon of the 16th he was prowling about, apparently for food, and ate voraciously when he obtained it; but it was soon after vomited unchanged, except that it was mixed with fluid. He also drank freely. He appeared pretty lively. On the 17th, he still ate and vomited the food unchanged. On the 18th, vomited some animal food unchanged, which had been retained for at least seven hours. On the 19th, vomited the food taken in the morning, and again swallowed it. 20th, No vomiting to-day, and food retained. 21st, Ate voraciously and apparently with relish, but food is vomited, and again partly swallowed. In the evening appeared duller than he had been for the two previous days. On the 22d was less lively, and appeared weak, having a considerable difficulty, when lying, in getting upon his legs, and occasionally staggering when walking. Almost constant cough. No vomiting to-day, except of some bread and milk about a minute after it was taken, which he again immediately ate up and retained. 23d, Less feeble than yesterday; still some vomiting, and the animal food vomited was partly in a pulpy state but without fœtor. 24th, Less appetite to-day, and some vomiting of animal food in a state of pulp. Is, however, regaining strength, and can walk, and even run without staggering. 25th, Ate voraciously to-day. Still occasional vomiting of half-digested food. Is rapidly gaining strength, and has less cough. 26th, Has eaten largely of animal food; no vomiting. 27th, This morning was very lively and active, and prowled about for food and water when liberated from the room in which he was confined; and leaped over a place between three and four feet high,

apparently for his amusement. Barks, but very feebly. At half-past eleven, A. M., he was fed with three-quarters of a pound of sweet butter, a small part of which he immediately vomited, and again swallowed. After this he made no efforts to vomit. About half-past three, P.M., he was killed by a dose of prussic acid, in the presence of Drs. Alison and Duncan and Mr. Spence, and the abdomen was immediately laid open. After the effects of the prussic acid began to manifest themselves, he vomited some of the butter in a fluid state. *Sectio.* The mesentery presented a beautiful display of innumerable lacteals filled with a white milky fluid; the thoracic duct was also full of chyle. Part of the butter, altered in its appearance, was found in the stomach and small intestines. The mucous membrane of the stomach presented a blush of red in the splenic extremity, and was of its natural thickness and consistence. The nerves were then carefully exposed in the neck, and it was ascertained by measurement, that between the cut ends of the left vagus, when the neck was not stretched, the distance was one inch and one line; and between the cut ends of the right vagus it was one inch and two lines. The distance between the cut ends of one recurrent was one inch, and between those of the nerve of the opposite side it was somewhat less. The cut ends of the *vagi* formed little bulgings, and there was no regeneration (as was to be expected) of the nerves. The lungs crepitated everywhere, and contained very little blood. Some adhesive mucus was found in several of the larger and smaller bronchial tubes. In one portion of the lung, which was about an inch in length and half an inch in breadth, the bronchial tubes appeared full of this adhesive mucus. The sympathetic was dissected in the chest, and, compared with a similar dissection in two dogs, was nearly of the same size. The nerve of the left side appeared somewhat larger than that of the opposite side, and also larger than the corresponding nerves in

the other two dogs, but this was not to any great extent. A portion of the mesentery, in which the chyle has been retained in the lacteals, and also the neck of the animal, showing the distance between the cut ends of the nerves, have been preserved.

We shall give some short details of the other three experiments. One of these was performed upon a full-grown middle-sized terrier bitch, and has been already referred to under the 10th Experiment. Both *vagi* and recurrents were divided, and a portion removed. The frequency of the respirations was diminished to one-half immediately after the division of the *vagi*, but they were performed with ease. The nerves were divided on the 22d of June, at eight o'clock, A.M. At ten, A.M., she appeared very dull and languid; and at four, P.M., when solicited to take food, refused to eat. On the 23d, took some bread and milk, which were soon after vomited. 24th, Took some bread and milk at half-past ten, A.M., and had not vomited at four, P.M. Part of it was vomited in the evening, but was scarcely changed. 25th, Took some animal food at eleven, A.M. Part of it was vomited unchanged between three and seven o'clock, P.M.; the rest was retained. 27th, Vomited part of the animal food taken yesterday, which was softened on the surface without fœtor. 28th, Is more lively, looks about her when she expects food, and eats with apparent relish. Part of the animal food (liver) taken yesterday had been vomited, and was partially digested—some of it being reduced to a brownish fluid; other portions were reduced to a pulp on the surface. All of it was without fœtor; and the altered portion permanently reddened litmus paper placed for a short time in contact with it. On the 29th and 30th, and the 1st of July, had very frequent vomiting of food, which she again swallowed only to vomit the greater part of it again. It was evident, however, that more food was swallowed than was rejected

by vomiting; was obviously becoming weaker, and was very lean. On the 2d July was found dead at eight, A.M. The central parts of the body were still warm, and the abdomen was immediately laid open. *Sectio.*—A few lacteals were observed on the mesentery, some of which were punctured, and a thick white fluid flowed out. The thoracic duct was then exposed, and found full of chyle, and a ligature was placed around it at the upper part of the chest. Another ligature was also placed around it at the lower part of the chest, and the duct punctured below it, when a thick white fluid flowed out. The nerves were then exposed in the neck; and it was ascertained that the distance between the cut ends of the *vagus* on one side was one inch two lines, and on the other side seven-eighths of an inch. The distance between the cut ends of the recurrents was at least one inch. The parts were left in this condition until they had been examined by Drs. Alison, Knox, Craigie, Handyside, Duncan, and Mr. Fergusson. The mucous membrane of the lungs was pale, and covered by a thin layer of mucus. There was no effusion into the air-cells, and the lungs were everywhere spongy, and contained very little blood. The stomach contained a considerable mass of hair, but no food; and its mucous surface was quite healthy. No softness or redness of cut ends of nerves. The preparation of the neck and the thorax, showing the distance between the cut ends of the nerves, and the thoracic duct full of chyle, is preserved in the University Museum. The subject of another experiment was an old and large pointer, already referred to under the 11th Experiment. The *vagi* were cut on the 27th June, at half-past eight, A.M. On the 28th, he was dull and languid; had no apparent uneasiness; refused to eat, and had a little vomiting. On the 29th, took a little food, which was soon after vomited. On the 30th, was very dull, and vomited the small quantity of food taken. 2d July, Took a quan-

tity of milk, which some time after was vomited in a coagulated state. 3d, Appetite deficient, and the food taken was in great part again vomited. On the 4th, the appetite had considerably improved; but a great part of the food taken was vomited after a longer or shorter time. On the 5th, still vomited a great part of the food taken, and was evidently becoming more feeble. At three, P. M., the state of his breathing showed that he was evidently beyond recovery; and he was poisoned by a dose of prussic acid. After the acid had begun to produce its effects, he vomited some half-digested bread and milk. *Sectio.*—The abdomen was immediately laid open in the presence of Dr. Patterson of Leith, and we were shortly after joined by Dr. Knox. Several lacteals were seen upon the mesentery full of chyle. Some of these were punctured, and a white thick fluid flowed out. The thoracic duct was now exposed, and found full of chyle. The pneumogastrics were then dissected in the presence of these gentlemen; and it was ascertained by measurement that the distance between the cut ends of the nerves were seven-eighths of an inch on one side, and one inch and one-eighth on the other, when the neck was in a semi-bent position. The stomach was quite healthy, and slightly red on the inner surface, as is usual when digestion is proceeding. The lungs, particularly the left, were considerably diseased, as we have already described under the head of morbid changes in the lungs. The fourth and last experiment was performed upon a full-grown and large mongrel, (between a pointer and terrier,) the subject of the 15th Experiment. The *vagi* and recurrents were divided on the 19th June, at three, P.M. The respiration was easy after the section of the nerves. On the 21st, vomited some liver taken the day before, which presented no decided change. Is more lively. At seven, P.M., ate some rabbit's flesh with apparent relish, but vomited it soon after. On the 22d, at four, P.M., vomited unchanged some rabbit's flesh taken at eight, A.M.

Is still languid and dull. On the 22d, coughed pretty frequently, and vomited all, or nearly all, the food taken. 24th, At ten, A.M., walked across the room to reach some bread and milk, of which he ate a considerable portion. Is generally dull and listless. At four, P.M., he had vomited part of the bread and milk. 25th, Is more lively; still coughs. Ate some liver at ten, A.M., which was not vomited at three, P.M. On returning at six, P.M., I found part of it had been vomited, and this was decidedly converted into a pulp on the surface. 26th, Took a considerable quantity of animal food (sheep intestine) with apparent relish. Some of it was vomited after eight hours, and was partly soft and easily lacerated, and partly reduced to a pulp, without emitting any disagreeable odour. 27th, Still vomits part of his food in various stages of digestion. 29th, Vomited some liver in a pulpy state, not putrid, and reddening permanently litmus paper. Very frequent cough, and discharge of mucus from the nostrils. 30th, Eats little, and vomits that little. In the evening refused food, and was found dead next morning. *Sectio.*—The distance between the cut ends of one *vagus* was one inch and two-eighths, and between those of the opposite side one inch. The distance between the cut ends of the recurrents was one inch and a half. The left lung was very considerably diseased, as we have already described.

We believe that no one who examines the details of the first of the four experiments we have here related, can for a moment doubt that the digestive process was fully reestablished before the death of the animal. Though the contents of the lacteals were not chemically examined, every one conversant with the recent history of physiology knows that late experiments have fully proved that the lacteals absorb none of the ingesta in any great quantity, except they have been previously converted into chyme in the stomach. Besides, as the animal was enclosed in a room

where nothing vomited could escape detection, I am confident that considerable quantities of food which he had taken into his stomach, during the last few days of his life, had disappeared from the digestive tube. Above all, the animal was undoubtedly gaining rapidly in flesh and in strength; and what is digestion but the name given to those actions by which the ingesta are converted into materials fit for ministering to the nutrition of the body? In the second Experiment, besides the evidence afforded by the vomiting of animal food partially digested, and capable of permanently reddening litmus paper, we discovered the presence of chyle in the lacteals after death. I am also equally confident that a considerable quantity of food had been retained in the stomach, notwithstanding the frequency of the vomiting; and this had disappeared from the digestive tube. In the third Experiment the milk was vomited in a coagulated state, and chyle was also found in the lacteals and thoracic duct. In the fourth Experiment we had also what I cannot regard but as unequivocal evidence of digestion, viz., the vomiting of half-digested food permanently reddening litmus paper, and the disappearance of a considerable quantity of food from the digestive tube. In the experiment of Arnemann, to which we formerly referred, digestion must have been re-established, since the animal lived 165 days. In the experiment mentioned by Sédillot, digestion must at least have been partially established, otherwise the animal could not have lived two months and a half. It is of importance to remark, that we only obtained distinct evidence of digestion in the four animals which lived beyond the fifth day, and that even in these the digestive process was at first completely arrested and only gradually improved. In the other thirteen experiments the animals either refused to take food, or the food when taken was either vomited or remained in the stomach unchanged. We believe, then, that we are justified in

concluding, that a deleterious influence is propagated downwards to the stomach by lesion of the *vagi*, yet that, if the animal live for a certain time, the digestive process may be re-established. Leuret and Lassaigne have detailed an experiment where digestion proceeded immediately after section of both *vagi* with loss of substance. They removed from three to four inches of each pneumogastric in a horse in perfect health, after fasting for four days. *Tracheotomy* was performed. After the operation the animal eat with appetite, and a ligature was then tied around the *œsophagus*. The animal was killed eight hours after eating. The stomach contained about the half of what it had eaten ; the rest had passed into the small intestines. That which remained in the stomach was chymified, as analysis proved. The lacteals on the mesentery were distended with chyle, which was found by analysis to consist of the usual ingredients. The thoracic ducts (in the horse there are two) were filled with chyle. M. Dupuy was present, and renounced his previously published opinions, "J'en conviens," a-t-il dit, "je m'étais trompé."[1] They also state that the same experiment was repeated with the same results.[2] Paetsch, in an inaugural dissertation,[3] transcribes part of a letter from Augustus Schultze, then Professor of Physiology at Fryburgh, in which it is stated that he fed two dogs with milk, and in one of these he cut the *vagi* on the *œsophagus*.[4] Both were killed an hour and a half after. In the dog operated upon, the milk was digested, and the small intestines contained chyme, and

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[1] Recherches Physiologiques et Chimiques pour servir à l'Histoire de la Digestion, pp. 133, 134. Paris, 1825.

[2] Oper. cit., p. 135.

[3] Dissertatio Inauguralis Physiologica sistens Experimenta quædam de Nervi Vagi in Digestionem Vi atque Potentia, p. 30. Gottingae, 1822.

[4] It is not expressly mentioned whether the œsophagus was at the same time divided or not, but we are led to infer that it was.

the lacteals were full of chyle. The digestion had not advanced so far in the dog not operated upon. He states that he satisfied himself by subsequent dissection that all the filaments of the nerves had been cut. From this single comparative experiment he rashly concludes that section of the *vagi* does not retard, but, on the other hand, accelerates the digestion by the irritation of the intestines. He further states, that he repeated this experiment with the same results, only the digestion had not proceeded so far.

Magendie states, that in experiments in which the *vagi* were cut upon the *œsophagus*, so as to avoid the disturbance of the respiration consequent upon section of the *vagi* in the neck, he found that the digestion was not arrested. Brachet objects to these experiments, that no one can be certain that all the filaments of the *œsophageal plexus* are divided, unless the *œsophagus* be at the same time cut across; and besides, Magendie does not state that he satisfied himself of the division of all the filaments, by dissection after death. Brachet affirms, that, on repeating this experiment, and taking the precaution of cutting the *œsophagus* across, he found that perfect digestion was prevented.[1] Paetsch in some experiments, cut the *œsophagus* across where it perforates the *diaphragm*, and found that the digestion of milk previously introduced into the stomach, was arrested.[2] That digestion should be arrested for a time after the serious injuries inflicted in performing such experiments is nothing more than what we should expect; and the more especially when we remember the instructive experiments of Brachet, where he found that the mere incisions necessary for laying

[1] Oper. cit., pp. 213-215.

[2] Oper. cit. Sir B. Brodie cut the *vagi* on the *œsophagus* in one experiment, and the digestion was arrested.

bare the *vagi* in the neck, though the nerves themselves were left uninjured, were sufficient, in some cases, to arrest the digestion of food previously taken into the stomach. Many experimenters, among whom we may enumerate Haller,[1] Brunn,[2] De Blainville,[3] Dumas,[4] Dupuy,[5] Legallois,[6] Macdonald,[7] Wilson Philip,[8] and Dr. Hastings,[9] have never observed evidence of digestion after lesion of the *vagi;* but such negative experiments cannot be considered at variance with the positive experiments we have stated; they only show, what every physiologist who has experimented much on this subject must, I think, be obliged to confess, that the digestive process is generally arrested after section of the *vagi* for the short time the animal usually lives; but they can never overthrow the results derived from positive experiments, provided these have been accurately performed, and are free from all sources of fallacy. On looking over my notes, I find that seven of the seventeen experiments were performed before I obtained any evidence of digestion, and that the four experiments in which this was observed followed each other almost in succession; and this may so far explain how some experimenters may be more successful than others,

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[1] Opera Minora, tom. i., pp. 359, 360. Experimenta 132, 135, 136. Haller states that he found the food in the stomach after section of the *vagi* putrid and converted into fæces.

[2] De Ligaturis Nervorum, Ludwig, tom. ii., Scrip. Nerv. Min. Sel., pp. 286, 287. 2d, 3d, and 6th Experiments.

[3] Propositions extraites d'un Essai sur la respiration. Paris, 1808.

[4] Oper. cit., p. 354.

[5] Oper. cit. Dupuy believed that animals appeared to die after section of the *vagi* from suspension of digestion. Müller (Physiology, p. 358) represents Dupuy's opinion on this question as very different from this; but we have only to refer to p. 365 of the Journal containing the memoir of Dupuy, in proof of the correctness of this statement.

[6] Oper. cit., p. 214.

[7] Dissertatio Experimenta quædam de Ciborum Concoctione Complectens. Edin. 1818.

[8] Oper. cit.

[9] Quarterly Journal of Science, &c., p. 45. 1821. Vol. xi.

when the number of their experiments is limited. It would appear that digestion is less impeded when the *vagi* are simply divided and their cut ends left in contact, than when they are separated from each other, or when a portion is removed. This interesting fact was elicited by the controversy between Dr. Wilson Philip and Mr. Broughton,[1] and appears to be so far confirmed by the experiments of Breschet, Milne Edwards, and Vavasseur.[2] In our experiments upon dogs, frequent vomiting invariably followed, for a certain time at least, the section of the nerves when food was taken into the stomach. This varied in degree in individual cases. In the 13th Experiment, (the first of the four experiments we have detailed above,) the vomiting had almost entirely disappeared. In the 10th Experiment, (the second experiment detailed above,) the vomiting was very frequent; and we have seen this animal vomit, and again eat up the food vomited, several times within a few minutes. The death of this animal was, we believe, to be attributed to the frequency of the vomiting, for we had evidence that digestion was proceeding at the time of death, and the lungs were quite sound. The vomiting after section of the *vagi* is, we believe, generally occasioned by the presence of food in the stomach exciting the sensation of nausea, and not from any irritation of the upper ends of the cut nerves. For I have repeatedly observed dogs that made no efforts to vomit when the stomach was empty, seized with vomiting immediately or soon after food was taken into the stomach. So readily is vomiting excited in dogs by substances taken into the stomach after section of the *vagi*, that, in injecting various substances, such as prussic acid, laudanum, alcohol, &c., for purposes to which we shall

[1] Quarterly Journal of Science, &c.. vol. xi. p. 320, and vol. xii. p. 17. 1822.

[2] Arch. Gén. de Méd., tom. ii. p. 485. 1823.

afterwards have occasion to refer, I found it necessary to tie the *œsophagus* with a ligature, to prevent their rejection from the stomach. It has nevertheless been stated that large doses of emetics do not produce their usual effects when injected into the stomach after section of the *vagi*.[1]

Effects of Lesion of the Vagi upon the Sensations of Hunger and Satiety.—Though these sensations are referred to the stomach as their seat, yet it is evident from well-established facts, that though they may be excited by impressions made on the expanded extremities of the nerves of the stomach, they are actually evolved in the encephalic portion of the nervous system. Are those impressions that excite the sensations of hunger and satiety, made upon the expanded gastric filaments of the *vagi*, or upon the filaments of the ganglionic system, or upon both? If they depend entirely upon impressions transmitted to the *medulla oblongata* through the *vagi*, it is obvious, that, after section of the *vagi* and recurrents in the neck, these sensations could no longer be felt; for it is one of the conditions absolutely necessary for the excitation of any particular sensation, that there be integrity of the nerve upon which it is dependent, throughout its whole course between the organ where the impression was made, and the central organs of the nervous system. If the sensations of hunger and satiety depend upon impressions made upon the gastric branches of the *vagi*, it is obvious, that when these nerves are divided in the neck, the stomach may be in the most proper condition for receiving impressions, and the *medulla oblongata*, where the nerve arises, may be in a fit state for the excitation of the sensation; yet it would be all to no purpose as long as the communication, through the nerves, between these two organs is interrupted. Brachet relates

[1] Brachet, Oper. cit., p. 187.

two experiments[1] to prove that the sensations of hunger and satiety are arrested by section of the *vagi*. From these he believes it is apparent that an animal, after section of the *vagi*, only eats to gratify the sense of taste, and that it continues to eat until the œsophagus and stomach are so much distended that it can swallow no more. The numerous experiments we have made do not by any means confirm these conclusions of Brachet. Though rabbits generally refuse to eat after section of the *vagi*, unless the food be placed under their nose, as in the experiments upon a dog and two Guinea pigs related by Brachet, yet I have seen one of these animals, immediately after the operation, make instantly for food, when this was thrown down in a corner of the room several feet distant from where it was at the time. Few of the dogs we operated on took food for the first twenty-four hours; and after all the coaxing we could practise, this was always in sparing quantities. Of the four dogs which lived beyond the fifth day, three cocked their ears, and looked out for food when we entered the room after they had fasted for several hours; and the quantity of food they took was variable at different times. In fact, we could not observe anything which would lead us to believe that they had lost the sensations of hunger and satiety. Of course it is quite possible that both these sensations might have been impaired by this operation; and it must be exceedingly difficult to ascertain how much an animal, in partaking of food, may be influenced by gratification of the sense of taste, and how much by the sensation of hunger. The mere retention of food in the œsophagus (the only proof adduced by Brachet) is no sufficient evidence that the food accumulates there from the over distension of the stomach caused by the loss of the sense of

[1] Oper. cit., Expériences 52, 53.

satiety; for in the rabbit, horse, sheep, and also occasionally in the dog,[1] this may arise from paralysis of the œsophagus.

We believe, then, that no sufficient evidence has been advanced to prove that section of the *vagi* annihilates the sensation of hunger and satiety; while, on the contrary, there are strong grounds for believing in their occasional continuance. And in all experiments of this kind, we must carefully separate the accidental from the necessary consequences of the injury of an organ. There are certain peculiarities in the excitation of the sensation of hunger, which are worthy of being kept in remembrance in judging of the effects of lesion of the *vagi* upon this sensation. If the sensation of hunger depend upon certain impressions being made upon the expanded filaments of the gastric nerves, we are naturally led to inquire into the causes of those impressions. In the case of the eye, for example, we find that, in the healthy state of the organ, we can always observe some excitation from the rays of light or other causes, by which the impressions are produced upon the optic nerve, that give rise to the sensation of sight. In the same manner, in the healthy state of the ear or the other external senses, we are familiar with the excitants by which the impressions made on the nerves belonging to those organs of sense, are usually produced. Can we then also point out any physical or chemical change acting upon the expanded filaments of the gastric nerves, which may be supposed to produce the impressions which excite the sensation of hunger? Many physical and chemical explanations of the origin of the sensation of hunger have doubtless been propounded, but they are all most unsatisfactory. It has been, as Magendie observes, by turns at-

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[1] *Vide* an experiment of Baglivi, formerly referred to.

tributed to the providence of the vital principle, to friction of the sides of the stomach against each other, to the dragging of the liver upon the diaphragm, to the action of the bile upon the stomach, to acrimony and acidity of the gastric juice, to fatigue of the contracted fibres of the stomach, to compression of the nerves of this viscus, &c. It would be mere waste of time to enter into any details to prove that each and all of the causes here adduced, as the excitant of this sensation, can have no such effect. We shall merely state, that some of these supposed causes cannot be present to excite this sensation; for example, there is no bile in the stomach in the natural state of that organ; and it is perfectly ascertained, more especially by the late researches of Dr. Beaumont, that no gastric juice is secreted, or at least contained in the stomach, during the empty condition of the organ. This sensation, then, is independent, as far as we know, of any physical condition of the stomach itself, or of the presence of any substance in its interior, but arises from certain organic changes in the constitution generally, connected with the necessity for additional supplies of fresh materials from without; but in what particular manner these conditions affect the expanded extremities of the gastric nerves so as to excite the sensation of hunger, is at present a perfect mystery. It may be supposed that, since various substances taken into the stomach allay the sensation of hunger, though they cannot pass into the body to form a part of the nutritious juices—and since various physical methods have been also found successful in producing the same effect, which cannot alter the conditions of the circulating fluid—we have sufficient proof that this sensation is entirely connected with some particular state of the filaments of the gastric nerves, and not upon causes developed in the constitution, as we have here supposed. It is, for example, well known, that travellers find relief from the pangs of hunger by swallowing

pebbles, or by compressing the abdominal organs by a tight girdle; and that some of the South American tribes swallow balls made of a kind of aluminous clay, when pressed by hunger. Even in an ordinary meal we are perfectly convinced that the sensation of hunger is completely allayed, and that, likewise, to the production of satiety, before the food has been in the slightest degree acted upon by the gastric juice. If this were not the case, this feeling of satiety would come too late to admonish us that enough has been taken into the stomach. All these, however, only succeed for a limited time in allaying this sensation; and if fresh chyle be not added to the blood, it will return, and become gradually aggravated in spite of all other attempts to relieve it. That these different methods should succeed for a time does not prove that this sensation is not excited by some organic cause developed in the constitution; for it is perfectly possible that, when the sensation is not in any great intensity, the particular condition of the filaments of the gastric nerves, induced by the organic causes developed in the constitution, whatever these may be, should be removed for the time by the physical effects of the several methods employed for allaying hunger. And this is borne out by the fact, that those succeed for a limited time only. Besides, it is also proved that the reception of food into the stomach, when not followed by the chymification and subsequent chylification of the food, does not even succeed in allaying the sensation of hunger after a time;—as is illus trated by the fact, that, in cases of disease of the pylorus, where assimilation does not take place to any great extent, the patient may feel very hungry with a full stomach. The importance of having this sensation allayed, when the wants of the system exciting it are not yet very urgent, by the physical circumstances which generally lead to the removal of those wants, even before the means for supplying them are actually furnished; and the advantage of the in-

sufficiency of all these methods to remove this sensation, when the wants of the system become very importunate, until the means themselves are actually applied to their removal, need only to be mentioned to be fully perceived. For it is obvious that, unless the sensation when not urgent was removed in the manner in which it is, it could no longer serve as a valuable guide in admonishing us to finish our repasts; while, on the other hand, when the supply of nutritious juices fairly begins to fail in the system, by refusing to be contented with anything less than a fresh supply of these, it forces the individual to look out for more nutritious materials.

Effects of Lesion of the Vagi upon the Secretions poured out on the inner surface of the Stomach.—We have, when discussing the effects of lesion of the *vagi* upon digestion, already detailed facts sufficient to show, that removal of a portion of both *vagi* and recurrents does not always arrest the digestive process, and consequently does not necessarily prevent the secretion of the gastric juice. In two of our experiments we ascertained that the half-digested food permanently reddened litmus paper; and we consider the presence of chyle in the lacteals and thoracic duct, as observed by Leuret and Lassaigne, and in three of our experiments, as furnishing decisive evidence of the secretion of gastric juice. In experiments by Dieckhof and Müller upon the effects of section of the *vagi* in geese, they found the fluid secreted in the stomach to be acid, but less so than in the sound animal. It is further stated, that they never found the acidity absent.[1] Mayer also found the chyme acid in rabbits after section of the *vagi*. Breschet, M. Edwards, and Vavasseur, Dr. Holland,[2] and Brachet, maintain that in their

[1] Müller's Physiology, vol. i., p. 594. Translated by Baly.

[2] Oper. cit., chap. x.

experiments the gastric juice was secreted, as the food in the stomach was more or less altered;[1] but many of these experiments are liable to certain objections, particularly where the animals were fed before the nerves were divided, for it is then impossible to decide how much gastric juice was secreted before the operation was completed. In the experiments of Breschet and Milne Edwards, in which they employed mechanical irritation of the lower ends of the nerves, the digestion was too far advanced to be explained by any supposition of this kind; for they state that, in one experiment, the alimentary matter was in a great degree reduced to pulp, the lacteals were full of chyle, and the digestion was as far advanced as when they employed the galvanic pile.[2] The positive proofs which we have here adduced of the secretion of the gastric juice cannot be invalidated by the negative results obtained by Tiedemann and Gmelin,[3] in an experiment upon a dog where the substances vomited after section of the *vagi* were found to be alkaline. As we believe that we have furnished adequate proof that secretion of the gastric juice may occur even when no mechanical or chemical irritation is applied to the lower extremities of the nerves, we have not thought it necessary to examine the supposed efficacy of galvanism, in supplying the want of the usual nervous influence transmitted along these nerves. We shall merely remark, that though it appears from the experiments of Wilson Philip,[4] Dr. Clarke Abel,[5] and Breschet, Vavasseur, and Milne Edwards,[6] that

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[1] The experiments of Mr. Broughton (Quarterly Journal of Science, &c. vol. x., p. 292,) are inconclusive, as the nerves were simply divided and the cut ends left in contact.

[2] Oper. cit., tom. vii., pp. 195, 196.

[3] Oper. cit.

[4] On the Vital Functions; and Quarterly Journal of Science, &c., vol. vii., p. 349; vol. viii., p. 72.

[5] Medical and Physical Journal, vol. xliii., p. 385. 1820.

[6] Oper. cit., tom. ii., p. 483, and tom. vii., p. 187.

galvanism transmitted along the cut *vagi* nerves to the stomach very much facilitates the digestive process, yet we are led to believe, from the experiments of Brachet, that it sometimes fails to do so, and Müller asserts that, in several experiments performed by himself and Dr. Dieckhof upon rabbits, the application of the galvanism in the mode recommended by Dr. Wilson Philip had no effect in promoting digestion.[1]

Secretion of Mucus.—We have examined the stomachs of many animals at various periods after section of the *vagi*, and we have never seen anything which would lead us to suppose that the usual mucous secretions are there arrested; on the other hand, we have every reason to believe that they were poured out in the usual quantity, and presented their usual physical properties. When the stomach was empty it was generally found contracted upon itself. The inner surface never presented the inflamed appearance described by Gendrin as the consequence of division of the *vagi*.[2] Sir B. Brodie relates four experiments in which animals were poisoned with arsenic after section of the *vagi*, where the usual watery and mucous secretions did not take place from the inner surface of the stomach and intestines, though the mucous membrane was inflamed.[3] In three of these experiments the *vagi* were cut in the neck, and in the fourth, they were divided on the cardiac orifice of the stomach, to avoid the effects of the operation upon the breathing. In three, the animals were poisoned by introducing ten grains of white oxide of arsenic into a wound

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[1] Elements of Physiology, 2d edit., vol. i., p. 598. *Vide* also Quarterly Journal of Science, &c., vol. vii., p. 161, 1819; and Medical and Physical Journal, vol. xliii., p. 250, for an unsuccessful repetition of Dr. W. Philip's experiments by three Members of the Royal Society.

[2] Histoire Anatomique des Inflammations, tom. i., p. 584. 1826.

[3] Phil. Trans., 1814, p. 102.

in the thigh; in the fourth, arsenious acid was dissolved and injected into the stomach. Sir B. thinks that the obvious conclusion from these experiments is, "that this secretion is prevented in consequence of the nervous influence having been interrupted by the division of the nerves of the eighth pair."[1] As it appeared improbable that section of the *vagi* should arrest the secretion of the mucus of the stomach and intestines, when it fails to suspend the secretion of the gastric juice, we were anxious to satisfy ourselves on this point. With this view we performed five comparative experiments, selecting two dogs as nearly as possible of equal size for each experiment. In four of these experiments the white oxide of arsenic was introduced into a wound in the thigh, in the quantity employed by Sir B. Brodie, and in the fifth it was introduced into the stomach. In each experiment the nerves were left untouched in one of the animals, while in the other both were divided and a portion removed, before the arsenic was introduced into the system.

38*th Exper.*—Two powerful and large dogs. The *vagi* and one of the recurrents were divided in one of the animals, and ten grains of white oxide of arsenic moistened with water were introduced into a long incision in the thigh, and the wound was then stitched up. The same quantity of arsenic was at the same time introduced into the thigh of the other dog, the *vagi* being left entire. The dog whose *vagi* had been divided appeared to be little affected three hours after the inoculation of the poison. Six hours after, it had vomiting of frothy mucus, and was very dull. Eleven hours after, it was found lying extended in a state of stupor, with frequent motions of the limbs. It was found dead next morning. The dog whose *vagi* were left entire was very slightly affected after six hours. After

[1] Oper. cit., p. 105.

eleven hours it was very dull, but was observant of surrounding objects. Twenty-three hours after, it was very feeble and languid, but still lay with the head erect. Other ten grains of arsenic were introduced into an incision made into the opposite thigh twenty-eight hours after the first inoculation of the arsenic. It was still alive, but very feeble and stupid. It was not seen for twelve hours after this time, when it was found dead and cold.

Sectiones.—Dog in which the vagi had been cut.—A quantity of greenish fluid flowed from the mouth, before we proceeded to open the chest. The stomach and a considerable portion of the small intestines were removed after being enclosed in ligatures, and then slit open. *Stomach.*—The stomach contained a considerable quantity of greenish fluid, similar to that which flowed from the mouth. The organ was somewhat distended, presented few *rugæ* on its inner surface, which was slightly tinged green. A thin layer of mucus could be wiped off in several parts. *Intestines.*—A thick layer of brownish mucus covered the whole of the inner surface of a great part of the small intestines. This mucus was in great abundance at the upper part, and at some places flowed from the intestine when held up, or even when one of the edges was somewhat raised. The mucous membrane itself was of a uniform red colour. The blood in the heart was coagulated.

Dog in which the vagi were not divided.—Stomach.—This organ also contained a greenish fluid, (apparently tinged with bile,) certainly not more abundant than in the other dog, but there were more flakes of mucus floating in it. Mucous surface corrugated and red in some parts. *Intestines.*—A thick layer of yellowish mucus covered the inner surface of a great portion of the small intestines, and the quantity did not differ decidedly from that observed in the former dog, when the two were compared together. The mucous membrane, however, in this animal was decidedly

redder and more thickened, and there were several patches of elevated glands of a bluish colour in the upper part of the small intestines, similar to those found in the ileum in the human species, but more rounded. Some decolorized clots of blood in the *venæ cavæ*.

Both animals had been fasting when the experiment was commenced, and were allowed neither solids nor fluids up to the time of death. The same precaution was adopted in the other four experiments.

39*th Exper.*—The previous experiment was repeated upon two small dogs. Three-and-a-half hours after the introduction of the poison neither animal seemed to be much affected, but the one whose nerves had been cut was vomiting a good deal of frothy white mucus, and eight hours after the commencement of the experiment it was found dead. The animal in which the nerves were left untouched was standing at the end of eight hours, but evidently dull, and was vomiting frothy mucus. In four hours more (or twelve hours from the commencement of the experiment) it was found dead.

Sectiones.—Dog in which the vagi had been cut.—Stomach. —The stomach was contracted, and contained some yellowish fluid with flakes of mucus floating in it; the mucous surface was corrugated and slightly reddened, and there was very little mucus adhering to it; no softening of the mucous membrane. *Intestines.*—A considerable portion of the small intestines was removed and slit open. A large quantity of thick yellow mucus lay on the inner surface, and could be partly made to flow out on holding up the edge of the intestine. The mucous membrane itself, particularly at the upper part, was thickened, reddened, and softened, and presented several elevated patches of glands. Much grumous blood in heart and large veins.

Dog in which the vagi were not divided.—Stomach.—This organ was contracted, contained some greenish fluid, and

its mucous membrane was more red than in the other dog. *Intestines.*—A very considerable quantity of reddish-brown thick mucus lined the inner surface of the upper portion of the small intestines, certainly presenting no decided difference in quantity from that in the former dog. The mucous membrane was reddened and softened, and the glands were elevated, of a blue colour, and surrounded by a red margin.

The blood was thick and grumous.

40*th Exper.*—Two stout dogs. The previous experiment was repeated. The dog whose *vagi* had been cut was found, twenty-three hours after the introduction of the arsenic, in a state of deep coma, and the respirations few and heaving. When visited the previous evening it had vomiting and diarrhœa. It died twenty-five hours after the commencement of the experiment. The other animal was dull and languid thirteen hours after the inoculation of the poison, but had not vomited. Twenty-four hours after, it was more dull and languid, had some diarrhœa, but no vomiting. Other ten grains of arsenic were introduced into the same thigh, and near the former incision. At the forty-eighth hour was very languid and feeble; diarrhœa continued, but without vomiting. Other ten grains of arsenic were introduced into the opposite thigh. At the fifty-second hour it was still without coma, though very feeble and languid. Its death was hastened by a blow on the head.

Sectiones.—Dog in which the vagi had been divided.—Stomach.—The stomach contained a small quantity of a brownish fluid; the mucous surface was corrugated, and of a dull-red colour throughout; only a small quantity of mucus in the stomach. *Intestines.*—The whole extent of the small intestines contained much brown fluid mucus, which ran out on slitting them open. Numerous small red patches presented themselves on the mucous surface,

particularly at the upper part; while in the intervals between these the membrane was of its natural appearance. Blood thick and grumous, with small soft coagula.

Other dog.—Stomach.—About twice the quantity of fluid found in the stomach of the former dog was contained in this. The mucous surface was thrown into numerous *rugæ*, was of a deep-red colour, and presented many dark elevated spots evidently from effused blood; some of these were of a greenish colour, somewhat like sloughs. *Intestines.*—The mucus found in the small intestines was not decidedly greater in quantity than in the former, but the mucous membrane was everywhere of a very bright red colour, evidently thickened and inflamed, though the glands were not apparent. The stomach and intestines of a dog killed twenty-four hours after division of the *vagi* were compared with those of the two animals employed in the experiments we have just detailed, and presented a marked difference in the quantity and physical appearances of the mucus found in the intestines.

We need not detail the other two comparative experiments, as they did not differ in any essential point from those we have related. In the 41st Experiment, two drachms of white oxide of arsenic were introduced into the stomach, and the œsophagus tied. The dog whose *vagi* were divided died in convulsions seven hours and thirty minutes after the commencement of the experiment. The dog whose *vagi* were left entire was seen alive, though feeble, at the end of eight hours, and was found dead two hours after this. The stomach was more inflamed in the latter than in the former, but there was evidently a greater quantity of effusion in the interior of the small intestines in the former than in the latter, which in both consisted of a whey-coloured fluid with flakes of mucus floating in it. In the 42d Experiment, the arsenic was introduced into a wound in the thigh. The dog in which the *vagi* were cut was seen alive

at the end of thirteen hours, and was found dead and cold at the end of twenty-four hours. At this time the dog in which the *vagi* were left entire was still alive, but was evidently very feeble, and had diarrhœa. The stomach in both of these animals was of a red colour, and contained some fluid. Much thick brown mucus flowed from the whole tract of the small intestines in the animal in which the *vagi* were not divided, and a thin greyish mucus from the other. The mucous surface of the small intestines was inflamed, especially in the upper part, but considerably more so in the animal in which the *vagi* were left entire than in the other. In three of these comparative experiments the stomach and intestines were slit open in presence of Dr. Alison. The stomach and intestines of the two animals, the subject of each experiment, were always examined at the same time, and in this manner were accurately compared with each other. Though the quantity of watery and mucous effusions from the inner surface of the digestive canal was apparently nearly the same in the two animals of the same experiment, and, in fact, presented no very marked difference in any of the ten animals killed in this manner, yet in all the experiments, except in the 39th Experiment, the mucous membrane of the stomach and intestines was more inflamed in the animals in which the *vagi* were not divided, than in those in which they had been previously cut. This, we believe, can be sufficiently accounted for by the circumstance, that the animals whose *vagi* were divided died generally several hours later than those whose *vagi* were left entire.

We cannot pretend to account for the discrepancy between the results obtained in the experiments detailed above and those related by Sir B. Brodie. It is possible, however, that Sir. B. had not taken the precaution of securing a free ingress of air into the lungs in the three experiments in which the nerves were cut in the neck, for it is stated that

in the first experiment, the breathing of the animal was labouring, and it died after three hours and a-half. In the third experiment the animal only lived three hours. In our experiments one animal whose *vagi* had been divided died after the eighth hour, and the others lived considerably longer. It must, however, be mentioned, that in the second experiment related by Sir B. the animal lived nine hours. Even in this case the death might have been hastened by the effects of the section of the nerves upon the muscular movements of the larynx. Whatever be the cause of this discrepancy, it is obvious that the negative evidence obtained by Sir B. Brodie cannot affect the value of the positive experiments we have detailed; and it necessarily follows that the conclusion deduced from it by Sir B. is not universally or even generally applicable; whilst we believe we are, on the other hand, justified in concluding, that lesion of the *vagi* does not necessarily prevent the usual watery and mucous secretions from the inner surface of the stomach and intestines. From all the experiments we have performed upon the effects of lesion of the *vagi* upon the different secretions poured out upon the inner surfaces of the stomach, we have arrived at the conclusion, that, if the agency of the nervous influence be at all necessary for the elimination of these from the blood, this may be affected by the ganglionic system after a portion of both *vagi* has been removed in the neck. We have also, we believe, adduced sufficient evidence to set aside all the arguments which have been drawn from the effects of lesion of the *vagi* nerves upon the functions of the stomach, by those who maintain that secretion depends upon the agency of nervous influence. Though the experiments we have stated cannot, of course, be adduced in proof of the opinion, that secretion is not necessarily dependent upon nervous influence, since other nervous filaments are distributed in the stomach—yet they nevertheless completely invalidate some of the argu-

ments which have been strongly insisted on by the supporters of the opposite doctrine.

The varied experiments upon the *vagi* ought to be viewed in two different aspects; for while they show that integrity of those nerves is not a condition absolutely necessary for the performance of secretion in the stomach, they yet prove that the secretions usually poured into the interior of that organ may be modified, in a most important manner, by causes acting through these nerves. We have here two perfectly separate and distinct propositions, which have sometimes not been clearly distinguished from each other. The difference between them may be illustrated in a very simple manner. The movements of a horse are independent of the rider on his back—in other words, the rider does not furnish the conditions necessary for the movements of the horse—but every one knows how much these movements may be influenced by the hand and heel of the rider. Though it would be out of place to enter here at any length upon the theory of secretion, yet we cannot avoid remarking, that the important influence which lesion of the *vagi* exerts upon the secretion of the gastric juice, must prevent us from attaching much importance to the negative experiments of Naveau,[1] Krimer,[2] Brachet,[3] and Müller,[4] upon the effects of lesion of the renal plexus of nerves upon the secretion of urine from the kidney. When we extend our investigation into the manner in which the function of secretion is performed, to the whole range of the vegetable and animal kingdoms,[5] we can, I think, have little difficulty

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[1] Dissertatio Inauguralis sistens Experimenta quædam circa Urinæ Secretionem, pp. 16-18. Halae, 1818.

[2] *Vide* Lund's Physiologische Resultate der Vivisectionen neuerer Zeit. S. 205.

[3] Oper cit., chap. v.

[4] Physiology translated by Baly, vol. i., p. 471.

[5] Carpenter's Principles of General and Comparative Physiology, chap. xi. 1839.

in arriving at the conclusion, that the nerves exercise over secretion "not an uniform and essential, but an occasional and controlling influence."[1]

Effects of Lesion of the Vagi upon the rapidity of Absorption from the inner surface of the Stomach.—It has been stated by Dupuy[2] and Brachet,[3] that after the *vagi* are divided, the most active poisons, introduced in much larger doses than usual, produce their effects more slowly, and with less energy. On the other hand, Müller asserts, that in thirty experiments on Mammalia, which M. Wernscheidt performed under his directions, "not the least difference could be perceived in the action of narcotic poisons introduced into the stomach, whether the *nervus vagus* had been divided on both sides or not, provided the animals were of the same species and size."[4] I have made several comparative experiments—the details of which it will be unnecessary to give—where I injected prussic acid, alcohol, and laudanum into the stomach, taking care, of course, that the two animals selected for each comparative experiment should be as nearly as possible of the same strength, and that equal quantities of the same poisons should be injected into the stomachs of both, and that the œsophagus should be tied by a ligature to prevent the rejection of the contents of the stomach by vomiting. In these experiments I could perceive no decided difference between the rapidity of the action of the poison in the animals whose *vagi* had been cut, and those whose *vagi* had been left entire. On the other hand, I have seen animals in which the *vagi* were uninjured recover from doses of narcotic poisons that proved fatal to other animals of equal or even superior strength, when these nerves had been previously divided. And this

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[1] Alison in vol. ix. of Quarterly Journal of Science, &c., p. 124. 1820.

[2] Oper. cit., p. 366.

[3] Oper. cit., p. 186.

[4] Physiology, p. 246.

is nothing more than what we should expect, when we remember that the effects of the diminished frequency of the respiration are conjoined to those of the narcotic poison.

Cause of Death after Section of the Vagi.—From what we have already said it is obvious, that though the animals in which the *vagi* have been divided generally die from morbid changes in the lungs, yet they occasionally die from inanition produced by the derangement of the digestive organs. The 10th Experiment is an example of the latter mode of death. We are of course here supposing that means have been taken to insure the access of a sufficient quantity of air into the lungs; otherwise the animal may die, suffocated from the arrestment of the movements of the muscles attached to the arytenoid cartilages.

From the numerous experiments we have made upon the *nervi vagi* and their branches, we have arrived at the following conclusions.

Pharyngeal Branches.—The pharyngeal branches of the *vagi* are entirely, or almost entirely, motor, and move the muscles of the pharynx and soft palate.

Laryngeal Branches.—The superior laryngeal are almost entirely nerves of sensation, supplying the mucous surface of the larynx, and part of the pharynx, with sensitive filaments. The few motor filaments which they contain are distributed in and move the crico-thyroid muscles. The inferior laryngeal nerves are ramified in and regulate the movements of all the muscles attached to the arytenoid cartilages, viz. the *crico-arytenoidei postici* and *laterales*, the *thyro-arytenoidei*, and *arytenoidei*. The inferior laryngeals also furnish the sensitive filaments to the upper part of the *trachea*, a few to the mucous surface of the pharynx, and still fewer to the larynx. When any irritation is applied to the mucous membrane of the larynx in the healthy state, this does not excite the contraction of the muscles that approximate the arytenoid cartilages, by acting directly

upon them through the mucous membrane; but this contraction takes place indirectly, and by a reflex action, in the performance of which the superior laryngeals act as the sensitive or afferent nerves, and the inferior laryngeals as the motor or efferent nerves. It is also probable that those branches of the inferior laryngeal distributed in the muscular fibres of the *trachea* are motor.

Œsophageal Branches.—The œsophageal filaments of the *vagus* are partly afferent and partly efferent nerves. In some animals, as in the rabbit, the section of the *vagi* in the neck is followed by arrestment of the passage of the food along the œsophagus, not from destroying the contractility of the muscular fibres of the œsophagus, but by breaking the continuity of the nervous circle necessary for the accomplishment of all reflex actions.

Cardiac Branches.—Though the movements of the heart may be materially influenced by causes acting through the *vagus*, yet mental emotions and injuries of the central organs of the nervous system affect the heart's action, after the *vagi* and recurrents are divided in the neck.

Pulmonary Branches.—The pulmonary branches of the *vagus* furnish the principal channel by which those impressions made at the lungs, capable of exciting the respiratory muscular movements, are transmitted to the *medulla oblongata*. It also appears that they transmit the impressions which excite the sensations of coughing, &c.; but we are inclined to believe that this may likewise be effected to a certain extent by the ganglionic system. It is probable also that the pulmonary branches contain motor filaments, though we have not been able to obtain any decided evidence of this.

Gastric Branches.—Lesion of the gastric branches does not necessarily arrest the muscular movements of the stomach, or the usual secretions poured out from its inner surface; though each and all of these, but particularly the

secretions, may be modified to a very important extent by causes acting through the nervous system.

[Since the preceding account of my experimental investigation into the functions of the glosso-pharyngeal, pneumogastric, and spinal accessory nerves, was drawn up, several competent observers have published the results they have obtained in similar researches; and though it is impossible to expect uniformity in investigations beset with so many sources of fallacy, yet, with a very few exceptions, the evidence they furnish preponderates very strongly in favour of the accuracy of the facts I have recorded, and the soundness of the conclusions I have deduced from these.

Some points of interest in the anatomy of these nerves, bearing upon their physiology, have been lately made the subject of discussion. Arguments in favour of the vagus being a motor as well as a sensiferous nerve, have been drawn from its anatomy. Remak[1] states that in the dog, the cat, and the rabbit, some of the filaments of the root of the vagus do not pass through its superior ganglion (*ganglion jugulare*); and Volkmann[2] confirms this observation of Remak as far as regards the dog, and farther mentions that the same arrangement exists in the sheep, while in the calf all the filaments of the root of the vagus pass through this ganglion. Mr. James Spence[3] observed, in dissections on the human species, a small filament, apparently belonging to the vagus, pass over the superior ganglion or *ganglion jugulare*, without entering into its formation, and then join itself to the upper fibres of the spinal accessory which run into the vagus immediately below this ganglion.

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[1] Froriep's Neue Notizen, &c., No. 54. 1837.
[2] Müller's Archiv. Jahrgang 1840. S. 490-1.
[3] Edinburgh Med. and Surg. Journal, No. 153. 1842.

The accuracy of such dissections, especially those made on the human species, and the physiological inferences drawn from them, have been called in question by Bischoff,[1] and Hein,[2] on the grounds that this anatomical arrangement is not constant, and, besides, that it is difficult to distinguish between the upper fibres of the root of the spinal accessory and the lower fibres of the root of the vagus. Bischoff states, as the result of dissections made both by himself and under his direction, that the portion of the root of the vagus described by Mr. Spence as passing over its *ganglion jugulare* is seldom present, and that it is entirely wanting in those animals on which experimental investigations upon this nerve have been conducted. Hein mentions that he has made an unsuccessful search upon more than ten heads for this alleged non-ganglionic portion of the vagus in the human species: while, on the other hand, he has more than once distinctly observed, as Krause has also remarked, the superior five or even eight filaments of the root of the accessory approximate closely to the *ganglion jugulare* of the vagus, and partly enter into its formation, so that a junction between the vagus and accessory had already taken place in this ganglion before the filaments of the accessory had fully collected themselves into a trunk about to divide itself into its two branches—internal and external. In such cases the short trunk of the accessory contains filaments which have partly entered into the formation of the superior ganglion of the vagus, and it cannot with certainty be said that none of these filaments originally appertain to the vagus.[3]

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[1] Bericht über die Fortschritte der Physiologie im Jahre 1842. S. 156. In Müller's Archiv. 1843.

[2] Müller's Archiv. Jahrgang 1844. S. 337.

[3] In making mention (p. 153) of some of the results of the dissections of Bendz, as they are given in Müller's Archives for 1837, and before I had procured a copy of Bendz's Monograph (De Connexu inter Nervum Vagum

One or more filaments generally pass between the *ganglion superius* or *ganglion jugulare* of the vagus and the spinal accessory nerve,[1] one filament, generally, between this ganglion and the *ganglion petrosum* of the glosso-pharyngeal, and another between it and the *superior ganglion* of the *sympathetic*.

We have seen that as the *vagus* and the *nervus accessorius* emerge from the *foramen lacerum posterius*, the internal branch of the *accessorius* joins itself to the vagus, and that while part of its filaments go to assist in forming the superior pharyngeal branch of the vagus, the remainder proceed downwards with the trunk of the vagus, and become incorporated with it.

Bendz states,[2] that the portion of the accessory which accompanies the vagus down the neck sends a few filaments to the upper part of the inferior or second ganglion of the vagus, and then joins itself to some of the posterior and external fibres of the vagus which do not pass through this ganglion. Below the ganglion these fibres form part of the trunk of the vagus, being enclosed in the same neurilema with those that pass through the ganglion. At the lower edge of the ganglion, or sometimes a little lower, the accessory portion sends off some filaments, which often join

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et Accessorium,) I state, that in the rabbit he had seen some of the filaments of the vagus pass over its *ganglion superius*. In reading the original Monograph I can find no statement to this effect; and as it is enclosed within brackets in the Archives I now perceive that the author of the Jahresbericht, in drawing up his epitome, had added this remark as the result of his own observation.

[1] Arnold in his Icones Nervorum Capitis, Tab. IV. Turici, 1834, figures a communicating filament between the *ganglion jugulare* of the vagus and the *nervus accessorius;* and Bendz (De Connexu inter Nervum Vagum et Accessorium—Willisii, Tab. I., Hauniæ, 1836,) figures two small communicating filaments passing between them.

[2] Opus cit., pp. 20-23. Longet (Recherches Expérimentales sur les Fonctions des Nerfs, des Muscles du Larynx, &c., p. 29, 1841,) states that he has not succeeded in tracing the filaments of the spinal accessory into the inferior laryngeals or recurrents.

the external branch of the superior laryngeal nerve, but it more frequently gives twigs to the sterno-thyroid muscle. Other fibres of the accessory portion accompany the vagus into the thorax, and some of them assist in forming the recurrent nerve. This accessory portion, after sending some filaments to the cardiac and pulmonary plexuses, proceeds onwards with the vagus to the stomach, where it is lost. Mr. Spence states that those fibres of the vagus which do not pass through the superior ganglion, or *ganglion jugulare*, are joined by the internal branch of the accessory; that these together form a small flat band, which may be traced among the other fibres of the vagus to the lower part of the neck, where it is joined by some of the other fibres of the vagus which have passed through the ganglion, and that it seems to go principally to the formation of the recurrent nerve.

We have seen that the *vagi* are distributed over a large space, and upon many organs. They send branches to the external ear, to the pharynx, the larynx, the œsophagus, the trachea, the thyroid body, the heart, the lungs, and the stomach; also to the liver, the spleen, the pancreas, the small intestines, and probably to the other viscera of the abdomen. In their course they communicate very freely and extensively with the sympathetic,[1] and to a greater or less extent with several of the cerebro-spinal nerves, as the spinal accessory, the glosso-pharyngeal, the hypoglossal, the portio dura, the two superior cervical, and occasionally with some of the lower cervical. The vagi are very extensively ramified upon the internal integumentary membrane, as the mucous membrane of the pharynx, larynx, œsophagus, stomach, trachea and lungs, and they send only

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[1] In many of the mammalia, the cervical portion of the sympathetic joins the trunk of the vagus immediately below the inferior ganglion of the vagus, and the trunks of the two nerves are there closely incorporated.

one small branch, viz. the *ramus auricularis*, to the external integumentary membrane. Many of its branches are distributed upon the muscular fibres surrounding the upper part of the digestive and respiratory tubes.[1]

According to Volkmann and Bidder, the vagi contain in all vertebrated animals a greater number of sympathetic than of cerebro-spinal filaments; and the preponderance of the sympathetic over the cerebro-spinal is more marked in the lower than in the higher vertebrata. This remark is in conformity with the observations of Dr. E. H. Weber,[2] upon the relative size of the vagus and sympathetic in the different families of the vertebrata, from which it appears that in the lower vertebrata the size of the vagus increases in proportion as the sympathetic diminishes in size, and *vice versa*. Volkmann and Bidder also state that the branches of the vagus distributed in the œsophagus, heart, lungs, stomach, liver, and gills, are chiefly composed of sympathetic filaments, while the recurrent, one of its motor branches, is chiefly composed of cerebro-spinal filaments.[3]

Numerous small ganglia have been discovered by Remak upon nervous filaments, distributed in organs whose supply of nerves is to a considerable extent derived from the vagi, but it would appear that these ganglia are placed upon the filaments of the sympathetic, that commingle with the vagi, and not upon those of the vagi. These ganglia have been observed by Remak, by the aid of the microscope, upon the filaments of the cardiac nerves where they are ramified upon the surface of the heart;[4] also upon some of

[1] We have given the anatomy of the vagus at considerable length in the article *Par Vagum*, in the Cyclopædia of Anatomy and Physiology.

[2] Anatomia Comparata Nervi Sympathici, pp. 77-82, et 101. Lipsiae, 1817.

[3] Die Selbstständigkeit des sympathischen Nervensystems, von Volkmann und Bidder. Leipzig, 1842; also Volkmann in article Nervenphysiologie in Wagner's Handwörterbuch der Physiologie.

[4] Casper's Wochenschrift für die gesammte Heilkunde den 9ten März, 1839.

the filaments of the pulmonary plexus, and upon some of the finer branches of the superior laryngeal nerves.[1]

The results of several investigations to ascertain the functions of the glosso-pharyngeal, vagus, and spinal accessory nerves by experiments upon their roots, and before any commingling, either of the fibres of these with each other or with the neighbouring nerves, has occurred, have lately been published. In the account I have given of the experiments I made to ascertain if the glosso-pharyngeal nerve contain motor filaments, I state that movements of the stylo-pharyngeus muscle were observed only in those cases where we had not satisfactory evidence that the irritation was confined to the filaments originally composing the glosso-pharyngeal nerve, and I therefore concluded that this nerve does not at its origin contain motor filaments. Valentin and Longet have since that arrived at the same conclusion from their experiments. Valentin relates that on irritating the roots of the glosso-pharyngeal within the cranium, after the removal of the encephalon in animals immediately after death, no muscular movements were ever observed, and he adds, "itaque fibras motorias primarie in nervo glosso-pharyngeo non inesse, demonstratur." He also observed effects similar to those we have described on irritating the pharyngeal portion of the glosso-pharyngeal, and the pharyngeal branch of the vagus, in the neck, "R. pharyngei N. glosso-pharyngei irritatio nunquam, ea R. pharyngei N. vagi, semper et certissime motus vehementes pharyngis ciet."[2]

Longet says, "J'ai galvanisé dans le but de provoquer des mouvements du pharynx, le nerf glosso-pharyngien avant son entrée dans le trou déchiré postérieur; aucune contraction de cet organe, ou des muscles qui l'avoisinent n'a

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[1] Medicinische Zeitung. Berlin, 8th Jan. 1840.

[2] De Functionibus Nervorum Cerebralium et Nervi Sympathici, p. 38, 1839 Valentin's experiments seem to have been chiefly performed on rabbits.

été vue, ni par moi ni par les personnes dont j'étais assisté."[1] On the other hand, Volkmann, Van Kempen, Hein, and Biffi and Morganti, have affirmed that muscular movements follow excitation of the root of the glosso-pharyngeal nerve, but they by no means agree in their accounts of the muscles moved. The results obtained by the different experimenters who have observed muscular movements follow excitation of the glosso-pharyngeus may be thus exhibited:—

Names of Observers.	Muscles thrown into contraction.
[2] Mayo, . . .	Stylo-pharyngeus, and the upper part of pharynx.
[3] Volkmann, . .	Stylo-pharyngeus, and middle constrictor of pharynx.
[4] Van Kempen, . .	Stylo-pharyngeus, and stylo-hyoid.
[5] Hein, . . .	Stylo-pharyngeus.
[6] Biffi and Morganti, .	Observed the uvula, velum pendulum palati, and both its anterior pillars or arches, quiver in consequence of the contraction of their muscles on irritating the nerve on one side.

These discrepant results forcibly point out the difficulty of arriving at the truth in such experiments, and this is not to be wondered at when we remember, that these nerves must be exposed as rapidly as possible, and before their excitability has disappeared, and that the filaments of the

[1] Anatomie et Physiologie du Système Nerveux, &c., tom. ii. p. 220. 1842.

[2] Anatomical and Physiological Commentaries, No. ii., pp. 11, 12. 1823. Mr. Mayo experimented on the nerve outside the cranium.

[3] Ueber die motorischen Wirkungen der Kopf-und Halsnerven. In Müller's Archiv. Jahrgang 1840, S. 475.

[4] Essai Expérimental sur la Nature Fonctionnelle du Nerf Pneumogastrique, &c. Louvain, 1842. Van Kempen, (opus cit., p. 34,) states, that in none of his experiments did he observe contractions of the middle constrictor of the pharynx, as Volkmann had done.

[5] Ueber die Nerven des Gaumenseegels. In Müller's Archiv. Jahrgang 1844, S. 297. Hein thinks that the constrictor isthmi faucium or palato-glossus muscle is moved through the nervus glosso-pharyngeus, but he could obtain no experimental evidence of this.

[6] Versuche am Nervus glosso-pharyngeus. In Müller's Archiv. Jahrgang 1847. S. 357.

glosso-pharyngeus and vagus lie close to each other, and are generally obscured by blood.

In some subsequent experiments which I made upon the effects of irritating the glosso-pharyngeus within the cranium in cats and rabbits, I certainly observed movements in the stylo-pharyngeus muscle when it appeared to me that the filaments of this nerve were alone embraced within the forceps, but circumstances at that time prevented me from prosecuting this matter farther. I am, therefore, now inclined, both from what I have observed myself, and from the results obtained by others, to believe that a few motor filaments are present in the glosso-pharyngeal nerve, and that these are chiefly, if not entirely, distributed to the stylo-pharyngeus muscle.

In the experiments I performed to ascertain the effects of irritating the roots of the vagus within the cranium, I observed muscular contractions both of the muscles of the pharynx, velum pendulum palati, larynx and œsophagus, and from these concluded that the vagus at its origin is a double nerve, containing both motor and sensiferous filaments. Volkmann, Van Kempen, Hein, Stilling, Wagner, Bernard, and among these, we may also now include Bischoff,[1] have arrived at the same conclusion from their experiments. Volkmann[2] on the application of galvanism to the root of the vagus within the cranium, perceived contractions in the following muscles:—the levator palati, azygos uvulæ, superior and inferior constrictors of the pharynx, palato-pharyngeus, crico-thyroid, the muscles attached to the arytenoid cartilages of the larynx, and the muscular fibres of the œsophagus.

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[1] Bischoff in his Bericht über die Fortschritte der Physiologie im Jahre 1842, in Müller's Archiv. Jahrgang 1843, S. 155-6, states that he observed movements of the velum pendulum palati, in which contractions of the levator palati muscles were very decided, on irritating either the roots of the vagus or nervus accessorius.

[2] Opus cit., p. 493.

Van Kempen on irritating this nerve within the cranium also by galvanism, saw contractions in all the three constrictor muscles of the pharynx, in the pharyngo-staphylin or palato-pharyngeus muscle, in the œsophagus, and in the intrinsic muscles of the larynx.[1] Hein perceived contractions in the elevator palati, azygos uvulæ and palato-pharyngeus, but in the last muscle less frequently than in the two former, on irritating the root of the vagus; and the same muscles were thrown into contraction on exciting the root of the spinal accessory.[2] Stilling states that his researches upon the vagus lead him to believe that it contains both motor and sensiferous filaments;[3] and in experiments upon two cats, he saw movements of the muscles of the pharynx, larynx, and stomach, follow excitation of the roots of this nerve.[4] Wagner does not enter into the details of his experiments, but simply states that they support the view that the root of the vagus contains both motor and sensiferous filaments.[5] Bernard's experiments were performed in a manner different from all those mentioned above. He destroyed the roots of the spinal accessory in live animals, and as certain movements of the larynx and pharynx were still retained in respiration and in swallowing, he concludes that the nervous filaments through which these muscular movements are effected, are derived from the vagus.[6] Valentin[7] and Longet[8] on the

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[1] Opus cit., p. 36. In stating these results of Van Kempen's experiments in the article *Par Vagum* in the Cyclopædia of Anatomy and Physiology, I have inadvertently written palato-glossus muscle instead of palato-pharyngeus.

[2] Opus cit., pp. 324, 325. It must be remembered that Hein's observations were restricted to the muscles of the palate.

[3] Ueber die Medulla Oblongata, S. 58. Erlangen, 1843.

[4] *Vide* Bischoff's Bericht, &c., S. 154. In Müller's Archiv. Jahrgang 1843.

[5] Lehrbuch der Physiologie. Dritte Abtheilung, S. 329. Leipzig, 1842.

[6] Recherches Expérimentales sur les Fonctions du Nerf Spinal, &c., in Archives Générales de Médecine, 4th series. Tom. iv., p. 397, and tom. v., p. 51. 1844.

[7] Opus supra cit., p. 45.

[8] Opus supra cit., tom. ii., pp. 263-266.

other hand, observed no muscular movements on irritating the vagus within the cranium. The strong proof we possess in favour of the presence of motor filaments in the root of the vagus, is quite sufficient to entitle us to set aside the negative evidence opposed to it, and to justify us in concluding that it, from the first, contains motor as well as sensiferous filaments.

The experiments lately made upon the spinal accessory, at least upon its upper filaments or the part of the root of this nerve which forms its internal branch, have also yielded conflicting results. All experimenters agree that irritation of the root of the spinal accessory causes contractions in the sterno-mastoid and trapezius muscles, proving beyond doubt that the external branch of this nerve is motor; but while some, as Volkmann, Van Kempen, and Stilling observed no muscular movements in the pharynx, larynx, and œsophagus, or in the organs where the internal branch of this nerve is distributed, others, as Valentin, Longet, Hein, Morganti,[1] Bischoff,[2] and Bernard, believe that they have obtained proof that the internal, equally with the external branch, contains motor filaments.

It is stated that while in some of my experiments irritation of the root of the spinal accessory within the cranium was attended by powerful convulsive movements of the shoulders, the muscles of the larynx and pharynx remained quiescent; yet in other cases contractions of these muscles accompanied those of the sterno-mastoid and trapezius. I concluded my account of these experiments by remarking, "that the internal branch of the spinal accessory assists in moving the muscles of the pharynx, we are satisfied, not only from the experiments just stated, but also from those upon the pharyngeal branch of the *vagus*.

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[1] Omodei, Annali Universali di Medicina. Juli 1843.

[2] Bericht, &c., in Müller's Archives for 1843, already quoted.

Of the probable destination and functions of the other filaments of the *internal branch* of the *accessory*, we cannot pretend to judge without more extended inquiries. We certainly do not consider that these experiments entitle us to assert that they are not motor." After a careful review of all that has been subsequently written on the functions of the *accessory* nerve, I see no reason to depart from the opinion here expressed, that the internal branch of this nerve does contain motor filaments, seeing that we have a sufficient amount of positive evidence in its favour. We believe then that the motor filaments, bound up in the trunk of the *vagus* in the neck are, in part originally contained in the root of the *vagus*, and are in part derived from the internal branch of the *accessory*. The difficulty of determining in many cases the line of demarcation between the filaments that form the roots of the *vagus* and *accessory*, conjoined perhaps with some anatomical differences in the arrangement of their motor and sensiferous filaments in different genera or species of animals, may serve as the probable explanation of the discrepant results obtained by physiologists from their experiments on the roots of these two nerves.

I have stated that I failed in three experiments in obtaining evidence of any contraction of the circumflexus or tensor palati muscle on irritating the third branch of the fifth pair of nerves within the cranium. Hein[1] in some of his experiments appears to have been more successful. As the contractions of this muscle impart a very slight movement, they may have escaped my notice, or they may not have occurred in these experiments, for in those de-

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[1] Müller's Archiv, 1844, S. 316, 325. Longet (opus cit., tom. ii., p. 190,) states in his résumé of the physiological actions of this nerve, that irritation of its small or motor root, "imprime de légers mouvements au voile du palais," but he does not detail the experiments upon which this inference is founded.

tailed by Hein they are described as being very slight in the first, and were not observed in some of the others. As the anatomical distribution of the motor part of the fifth pair of nerves is in favour of the view that the tensor palati muscle is moved through this nerve, I am inclined to believe in its accuracy.

Mr. Shaw[1] relates two cases of paralysis of the portio dura in the human species, from lesion of the trunk of this nerve within the temporal bone, where partial paralysis of the velum pendulum palati, accompanied the usual paralysis of the muscles of expression of that side of the face; and some analogous cases have occurred in the Parisian Hospitals.[2] Valentin in five experiments upon animals immediately after death, irritated the portio dura within the cranium, with the view of ascertaining if this nerve exercised any motive influence upon the muscles of the soft palate, and in one case only did he observe any motion of these muscles, and that so indistinctly, as to render the experiment unsatisfactory.[3] M. Debrou has also performed the same experiment five times, and in one of these saw distinct movements of the muscles of the palate.[4] Volkmann[5] and Hein[6] failed completely in detecting any movements of the palatine muscles in irritating the portio dura within the cranium.

Those who believe that the portio dura nerve can move the palatine muscles, suppose that the *ramus petrosi superficialis major*, running between the portio dura and the *spheno-palatine ganglion*, is chiefly composed of nervous filaments from the portio dura, which, after passing through this

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[1] Medical Gazette for September, 1837.
[2] *Vide* Longet in opus cit., tom. ii., p. 450.
[3] De Functionibus Nervorum Cerebralium, &c., p. 33.
[4] Thèse Inaug., p. 22, et suiv., as quoted by Longet.
[5] Müller's Archiv. Jahrgang 1840. S. 485-487.
[6] Müller's Archiv. Jahrgang 1844. S. 316-325, und S. 331-333.

ganglion, in the same manner as the motor branches of the third pair of nerves going to the iris pass through the *ganglion opthalmicum*, join themselves to the *posterior palatine* branches of nerves proceeding to the soft palate. We do not, however, under all the circumstances of the case, consider the evidence in favour of the distribution of motor filaments derived from the portio dura, among the muscles of the velum pendulum palati, sufficiently strong to justify us in adopting this opinion.

We have seen that there are strong grounds for believing that the glosso-pharyngeal and motor part of the fifth pair move at least two of the muscles engaged in deglutition, viz. the stylo-pharyngeus and tensor palati, so that in accordance with this view the enumeration I have given at p. 166, of the afferent and efferent nerves of deglutition, should be extended as follows:—In the performance of the function of deglutition, the *impressions* are conveyed to the *medulla oblongata*, along the branches of the glosso-pharyngeal, and of the fifth pair distributed upon the *fauces*, and also probably along these branches of the superior laryngeal nerve distributed upon the pharynx. The *motive influence* transmitted outwards from the *medulla oblongata* passes, we believe, along the pharyngeal branches of the vagus, and a few of the filaments of the glosso-pharyngeal; along the branches of the hypoglossal distributed upon the muscles of the tongue, the sterno-hyoid and sterno-thyroid muscles; along the motor filaments of the recurrent, ramified on the muscles of the larynx; along some of the branches of the fifth pair supplying the elevator muscles of the jaw, and the tensor palati muscle; along the branches of the *portio dura* ramifying upon the digastric and stylo-hyoid muscles, and the muscles of the lower part of the face; and probably along some of the branches of the cervical plexus, which unite themselves to the *descendens noni* nerve.

Having thus finished a review of the experiments lately made to ascertain the functions of the glosso-pharyngeal, vagus, and spinal accessory, by irritation of their roots within the cranium, we shall now proceed to examine, in a similar manner, the results of experiments made upon the trunks and branches of these nerves, after their exit from the cranium.

Glosso-pharyngeal.—In our experiments on the glosso-pharyngeal nerve we obtained satisfactory evidence that it contains sensiferous filaments, and this has been confirmed by the experiments of Valentin,[1] of MM. Jules Guyot and Cazalis,[2] and of Longet.[3] Guyot and Cazalis[4] and Longet,[5] seem also to have witnessed the reflex movements that follow excitation of the trunk of this nerve. We also pointed out that division of both glosso-pharyngei with loss of substance does not impede the muscular movements of deglutition, and explained that the effects upon this function observed in Dr. Alcock's experiments, must have arisen from lesion of the pharyngeal branch of the vagus; and Longet[6] has fully confirmed these results. The difficulty of deglutition after lesion of these nerves, mentioned by Guyot and Cazalis and Magendie,[7] must have arisen from the same cause as in Dr. Alcock's experiments. We have pointed out that the glosso-pharyngeal and the pharyngeal branch of the vagus lie so closely together in the neck, that great care is necessary to avoid lesion of the one while experimenting on the other.

The share exercised by the glosso-pharyngeal nerve in the performance of the function of taste, has continued to

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[1] Opus cit., p. 39.

[2] Recherches sur les Nerfs du Goût. In Archives Générales de Médecine, tom. xlix. 1839.

[3] Opus cit., tom. ii., p. 222.

[4] Opus cit., p. 258.

[5] Opus cit., p. 222.

[6] Opus cit., p. 228.

[7] Leçons sur les Fonctions et Maladies du Système Nerveux, tom. ii., p. 293. 1839.

excite considerable interest, and a difference of opinion is still to be found on this point. We have detailed some experiments, which seem to prove that the sense of taste is not destroyed by lesion of the glosso-pharyngei, and we stated that similar results had been previously obtained by Dr. Alcock and by Dr. Kornfeld.[1] We might also have mentioned that Mr. Mayo[2] had adduced several objections to the conclusions of Panizza, that the glosso-pharyngei are the special nerves of the sense of taste. He rests his reasons of dissent principally upon the fact, that the distribution of this nerve is confined to the posterior part of the tongue, while the sense of taste is also present in the anterior part of that organ, and consequently it cannot, in that portion at least, depend upon the glosso-pharyngei. Mayo attempted to decide this question by experiment, but he did not carry this sufficiently far to obtain any satisfactory results. The persistence of the sense of taste after section of the 3d branch of the fifth pair, may, Mr. Mayo supposes, depend upon the palatine twigs of the 2d branch of the fifth pair distributed upon the palate and isthmus of the fauces. The conclusions of Panizza regarding the dependence of the sense of taste upon the glosso-pharyngei have derived support from the experiments of Valentin[3] and Bruns;[4] but are at variance with the results obtained in the experiments of Guyot and Cazalis,[5] of Magendie[6] and of Longet.[7]

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[1] Müller and Gurlt were associated with Dr. Kornfeld in the performance of these experiments. *Vide* Müller's Jahresbericht über die Fortschritte der anatomisch-physiologischen Wissenschaften, in his Archives for 1837, p. 134: also Kornfeld "De Functionibus Nervorum Linguæ Experimenta." Berol. 1836.

[2] Medical Gazette, 3d October 1835, and 4th edition of his Outlines of Physiology, p. 314.

[3] Repertorium, Band ii., S. 225: and De Functionibus Nervorum Cerebralium, &c., p. 41. 1839.

[4] De Nervis Cetaceorum. Tüb. 1836, as quoted by Müller. [5] Opus cit.

[6] Leçons sur les Fonctions du Système Nerveux, tom. ii. 1839.

[7] Opus cit., tom. ii., pp. 225, 226. 1842.

Bidder divided the glosso-pharyngei in two dogs, and though they swallowed pieces of flesh soaked in an infusion of colocynth, which another dog, whose nerves were uninjured, at once rejected, yet certain movements of the lips and tongue were observed, that showed that these were not altogether relished.[1] These were probably the experiments witnessed by Volkmann, and which appeared to him to furnish no decisive proof in favour of Panizza's doctrine.[2]

The evidence bearing upon this question, obtained from observations made in cases of paralysis of the fifth pair of nerves in the human species, is like that furnished by experiments upon animals, apparently contradictory, but when more narrowly examined, it preponderates decidedly, we believe, in favour of the opinion that the glosso-pharyngei are not the sole nerves of taste. In the single cases observed and recorded by Mayo,[3] Serres,[4] Romberg,[5] Mr. Bishop,[6] and Todd and Bowman,[7] and in the two cases by Mr. Dixon,[8] both common sensation and the sense of taste were annihilated in those parts of the tongue supplied by the fifth pair; while in one case related by Mr. Noble,[9] and another by Vogt,[10] common sensation was lost in the parts

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[1] Article *Schmecken*, in Wagner's Handwörterbuch der Physiologie, Bd. iii., S. 6-7. 1846. Though Bidder is favourable to the opinion that the glosso-pharyngei contain the specific nervous filaments of the sense of taste, yet he admits that his experiments did not furnish him with results so decided as those of the same kind communicated by Panizza and Valentin—" haben mir nie so entscheidende Resultate gegeben als Panizza und Valentin dergleichen mittheilen."

[2] " Nachdem, was ich bei Bidder zu sehen Gelegenheit hatte, würde ich die Vivisection wenigstens nicht für einen entscheidenden Beweis zu Gunsten Panizza's anzusehen wagen." Article *Nervenphysiologie*, in Wagner's Handwörterbuch der Physiologie. 1845.

[3] Anatomical and Physiological Commentaries, No. ii., p. 12.

[4] Anatomie Comparée du Cerveau, tom. ii., pp, 67-87. 1827.

[5] Müller's Archiv. Jahrgang 1838. S. 305.

[6] Medical Gazette, December 1835.

[7] Physiological Anatomy, vol. i. part ii., p. 445.

[8] London Medico-Chirurgical Transactions, vol. xxviii., p. 373. 1845.

[9] Medical Gazette, October 25th, 1834, p. 120.

[10] Müller's Archiv. 1840. S. 72.

of the tongue supplied by the 3d branch of the fifth pair, yet the sense of taste remained in these parts; and in a second case related by Noble,[1] there was loss of taste with maintenance of feeling.

We have no proof that in the cases related by Mr. Noble and Vogt, the whole of the filaments of the fifth pair sent to the tongue were affected; and in the case of Vogt, the derangement of the nerve was only temporary, for the patient recovered the sensation of the part paralyzed after the end of six weeks. We believe that these cases of Mr. Noble and Vogt, when examined more closely, will rather be regarded as affording arguments in favour of the opinion that the same nervous filaments do not convey inwards the impressions which excite pain and touch, and the impressions which excite taste, and that different filaments for conveying inwards the impressions that excite these sensa-

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[1] Medical Gazette, 21st November 1835, p. 259; also December 1835. Of the cases mentioned, where both common sensation and the sense of taste were lost in the parts of the tongue supplied by the fifth pair, those of Mr. Mayo, Serres, and Mr. Bishop, are less satisfactory than the other four. In Mr. Mayo's case, the root of the tongue on the paralyzed side was also insensible to touch and taste—a proof, in our opinion, that the glosso-pharyngeal was not in a normal condition. In Mr. Bishop's case, though after death the tumor found enveloping the three branches of the fifth pair within the cranium, and obliterating the foramina rotundum and ovale, did not apparently involve the glosso-pharyngeus or vagus, yet the latter, at least, of these nerves, had not remained unaffected, since the patient before death had extreme difficulty in swallowing. In Serres' case, the functions of the organs of external sense upon which the branches of the fifth pair are distributed, but which derive their nerves of special sensation from other senses, were so seriously deranged, that it is of little use in enabling us to decide how far the sense of taste is dependent upon the fifth pair. In one of Mr. Dixon's cases (Medico-Chir. Transact. 29th vol., p. 131,) the whole trunk of the fifth pair was involved in a tumor; and in Romberg's case, the 3d branch of the fifth pair was found diseased; and we have no reason to believe that any of the other nerves were affected. There is no case on record, as far as I am aware, where the 3d branch of the fifth pair *was found diseased after death,* where during life the sense of taste was retained in the anterior and middle parts of the tongue. In several of the cases of paralysis of the fifth pair recorded, nothing is said of the condition of the sense of taste.

tions are bound up in the lingual branch of the fifth pair. Many analogical arguments could be adduced in favour of this view. We know, for example, that though the sensations of heat and cold are chiefly confined to the surfaces where the sense of touch is exercised, yet we find that these sensations do not bear any direct relation to each other, or to the sense of touch, in regard to intensity, and that when the one becomes preternaturally exalted or obtuse, the other does not necessarily participate in these changes. Cases are on record where the sensation of heat was so exalted that cold bodies felt hot, and without any apparent change in the other sensations referred to the external surface of the body.

While then we maintain that the lingual branch of the fifth pair can convey inwards to the central organs of the nervous system those impressions which excite the sensation of taste, we are also satisfied that filaments of the glosso-pharyngeal perform a similar function in a higher degree, and on the following grounds:—

1. The sense of taste at the base of the tongue is not destroyed by lesion of the fifth pair.

2. The glosso-pharyngeal is the only sensiferous nerve distributed in the *papillae circumvallatae*, where the sense of taste is felt in greatest intensity.

It appears from the experiments of Horn,[1] Picht,[2] and of Guyot and Admyrauld,[3] that there are various substances which excite different sensations of taste when applied to the apex and to the base of the tongue; in other words, to those parts supplied by the fifth pair and to those parts supplied by the glosso-pharyngeal. It would also appear that the sensations of nausea caused by certain excitations of

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[1] Ueber den Geschmacksinn des Menschen, &c. Heidelberg, 1825.

[2] De Gustus et Olfactus Nexu, &c. Berolini, 1829.

[3] Archives Générales de Médecine. 3ième série, tom. i., p. 51. 1837.

the back part of the tongue and fauces, are chiefly dependant upon the glosso-pharyngeal.

Auricular Branch of the Vagus.—From the distribution of this small branch upon the integuments of the pavilion of the ear, and upon the lining membrane of the meatus auditorius externus, we may conclude that it is composed of sensiferous filaments. Valentin,[1] on irritating this branch in a rabbit, observed no movements of the muscles of the external ear. Arnold attributes the sympathy occasionally observed between the external ear and the lungs to this branch of the vagus. He refers to cases where the presence of hardened cerumen—of a bean, of a pea, and other foreign bodies, within the cartilaginous tube of the external ear, has induced long continued cough and even vomiting.[2] In many individuals cough can readily be induced by excitation of the inner surface of the meatus auditorius externus. If these reflex movements are induced through the auricular branch of the vagus—as there are good grounds for believing—the excitations which give rise to these actions will be conveyed inwards to the medulla oblongata, and cause the same changes there, as occur in those cases where coughing or vomiting are produced by excitations applied to the inner surface of the lungs and stomach.

Pharyngeal Branch of the Vagus.—That excitation of the pharyngeal branch of the vagus causes extensive movements of the pharynx, has been confirmed by the experiments of Valentin[3] and Longet; and it has been already remarked, my experiments upon the effects of lesion of

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[1] De Functionibus Nervorum Cerebralium, &c., p. 46.

[2] Bemerkungen über den Baue des Hirns und Rückenmarks, &c., S. 168. Zurich, 1838.

[3] Opus cit. Valentin in describing the effects of excitation of the pharyngeal branches says, " nam iis excitatis pharynx eximie a superiori ad inferiora contrahitur," p. 47.

these nerves upon the muscular movements of the pharynx in deglutition, have been fully confirmed by the experiments of Longet.

Laryngeal Branches of the Vagus.[1]—Mr. Hilton[2] has, from the anatomical distribution alone of the laryngeal nerves, arrived at the conclusion that the superior is chiefly sensitive, and that the only motor filaments which it contains are distributed in the crico-thyroid muscle; while the inferior laryngeal or recurrent supplies all the muscles that move the arytenoid cartilages, with motor filaments—a view in exact accordance with that we have given above. Valentin, after stating that the superior laryngeal is a mixed nerve, chiefly, however, composed of sensiferous filaments, adds—" sin enim in cadavere equi irritatur, minores laryngis musculi convelluntur."[3] If Valentin had extended his experiments on this nerve, he must have satisfied himself that the only intrinsic muscle of the larynx moved by the superior laryngeal nerve is the crico-thyroid.

Valentin gives the following short account of the experimental history of the superior laryngeal nerve, after stating the results of his own experiments quoted above—" Ita etiam Bischoff post R. laryngei superioris in cadavere factam irritationem tunicam mucosam laryngis moveri, et Magendie animalia in quibus R. uterque laryngeus superior erat divisus, sonos acutos non amplius proferre observavit. Krimer quum R. laryngeus superior acu irritaretur, cartilagines rimæ glottidis tremere, rimam ipsam

[1] An epitome of my first paper on the functions of the eighth pair, including the experiments upon the laryngeal nerves, along with observations upon their anatomy, and deductions from these, was read at the meeting of the British Scientific Association in 1837, and an account of these, still farther abridged, but containing the inferences regarding their functions, was published in the Athenæum for 16th September 1837.

[2] Guy's Hospital Reports for October 1837, forming part of the 2d volume.

[3] De Functionibus Nervorum Cerebralium, &c., p. 47. 1838.

aperiri nec, ut in respirando fieri solet, alternatim aperiri claudique videt. Animal raucos sonos edit." Volkmann,[1] on irritating the superior laryngeal nerve, saw movements "in the crico-thyroid, constrictor faucium supremus, and in dogs and calves also in the hyothyroid (probably from the intermixed fibres of other nerves), but this had no effect upon the muscles that move the arytenoid cartilages." In two young dogs he removed the brain and cerebellum, and freely exposed the larynx; and on cutting the superior laryngeal nerves in one of these animals, he observed no effects upon the movements of the glottis, while on dividing, in the other animal, the vagi, so as to cut off the influence of the recurrents, all these movements were arrested. He details other experiments, which go to confirm the views we have advanced regarding the relative influence of the laryngeal nerves in moving the intrinsic muscles of the larynx.[2] The experiments of Longet,[3] of Van Kempen,[4] and of Traube,[5] give additional support to the conclusions at which we have arrived regarding the functions of these nerves.[6] Longet[7] states that the division of both recurrents increases the frequency of the respiratory movements—in dogs from 18 to 20 per minute (the normal number), to 30 and 32, and in rabbits from 60 to 70 per

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[1] Müller's Archiv. Jahrgang 1840. S. 494. [2] Opus cit., pp. 495-6.

[3] Recherches Expérimentales sur les Fonctions des Nerfs, des Muscles du Larynx, &c. Paris, 1841. In several works lately published on the Continent, the credit of first satisfactorily determining the relative share of the laryngeal nerves in moving the intrinsic muscles of the larynx, is given to Longet, though my experiments, which are even more complete on this point than those of Longet, were published between three and four years earlier.

[4] Essai Expérimental, &c., pp. 53-8. Louvain, 1842.

[5] Beiträge zur experimentallen Pathologie und Physiologie. S. 95-108. Erstes Heft. Berlin, 1846.

[6] *Vide* p. 115 of this volume.

[7] Recherches Expérimentales sur les Fonctions des Nerfs, des Muscles du Larynx, &c., pp. 21-22.

minute, to 100 or even 108—and this in consequence of the diminution of the size of the superior aperture of the larynx reducing the bulk of the stream of air passing in and out of the lungs. Mendelssohn,[1] on the other hand, states, that in his experiments on rabbits, a considerable diminution in the frequency of the respiratory muscular movements followed the division of both recurrents—a result we certainly would not *a priori* expect. Traube[2] has repeated these experiments, and has arrived at the conclusion that Longet's statements are correct as far as they relate to dogs, but in the case of rabbits that the effects vary, and that neither the results obtained by Longet nor by Mendelssohn are constant. From what we have observed in our own experiments, and from consideration of those detailed by the authors mentioned above, we are satisfied that the effects of section of both recurrents upon the frequency of the respiratory movements, are chiefly influenced by the relation of the size of the larynx, and the quantity of air required to carry on the function of respiration in the particular animals operated on.

Longet found that section of the inferior laryngeals or recurrents produces complete aphonia in old dogs, while in the case of young dogs they could still emit acute sounds different from the natural voice, provided the movements of the crico-thyroid muscles, dependent upon the external branch of the superior laryngeal, be not arrested, and this difference he attributes to the relative size of the larynx at these different ages. Longet also states that division of the superior laryngeals above the origin of their external branch, or of this external branch alone, is followed by a disagreeable hoarseness of the voice, and he believes that

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[1] Der Mechanismus der Respiration und Cirkulation, &c., pp. 30-37. Berlin, 1845.

[2] Opus cit., 95-103.

this arises from the effects which paralysis of the crico-thyroid muscles has upon the vocal chords—of which chords these muscles are, according to his observations, tensors. Van Kempen, also, in his experiments, observed the vocal chords become stretched during the contraction of the crico-thyroid muscles.[1]

A considerable number of cases have been published where one or both recurrent nerves have been compressed by aneurisms or morbid growths, which illustrate the physiology of these nerves. I have seen two or three cases of partial aphonia, from pressure of the left recurrent nerve, from an aneurism of the arch of the aorta, in one of which the patient had been treated for an affection of the larynx, by blistering, &c. I have in my possession a preparation procured from the body of a young man who died very suddenly, with all the symptoms of suffocation, when seated with some companions round a fire, who were chatting and laughing. Both recurrents are embedded in a firm yellowish tumour, through which they cannot be traced.[2]

Œsophageal Branches.—Arnold in his experiments upon hens and pigeons, observed the œsophagus and crop so relaxed after section of the vagi, that when these animals shook their head and neck, or kept the head in a depending position, a quantity of chyme flowed from the bill.[3] Van Kempen concludes from his experiments, that the movements of the œsophagus are completely under the control of the vagi, and that its movements in deglutition are arrested by division of these nerves.[4] In my experiments, I found that the effects of lesion of the œsophageal

[1] Opus cit., pp. 53-4.

[2] This case is one of those referred to by Dr. Henderson in the Edinburgh Monthly Journal of Medical Science, vol. i., p. 16.

[3] Bemerkungen über den Baue des Hirns, &c. S. 144.

[4] Opus cit., pp. 58-63. Van Kempen's experiments were performed on rabbits.

branches of the vagus, are not the same in rabbits and dogs; and it would appear that the opinion lately expressed by Dr. M. Hall, that in some animals the muscular contractions of the œsophagus in deglutition are exito-motory, while in others they are called into action by direct excitation, is correct. We cannot at present determine whether the propulsion of the food along the œsophagus in the human species, partakes more of the former or of the latter class of movements.

It appears that the physical structure of the muscular fibres of the œsophagus, differs in the animals upon which physiological experiments are more generally performed; for according to the Brothers Weber,[1] both the longitudinal and circular fibres of the œsophagus of the rabbit present the appearances which have been termed *striae*, and the other physical characters of the muscles of voluntary motion, while those of birds, as the hen and pigeon, are *non-striated*, and have the physical structure of those muscles that have been termed organic. The application of electro-galvanism to the œsophagus in these animals, exhibits the usual distinction in the contractile properties of these two kinds of muscular fibre, when subjected to that mode of excitation. In the œsophagus of the dog and cat on the other hand, these two kinds of muscular fibre are conjoined, for while the longitudinal fibres through the whole extent of this tube, and the circular fibres in its upper half have the physical structure and the vital properties of the muscles of voluntary motion, the circular fibres in its lower half resemble the organic muscles in these respects. These differences will not, however, enable us to explain the different effects of lesion of the vagi, upon the movements of the œsophagus in deglutition; they do not, for example,

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[1] Wagner's Handwörterbuch der Physiologie. *Article* Muskelbewegung. Bd. iii. S. 30-1, 1846.

explain how the œsophagus should be paralyzed in rabbits, in hens and in pigeons, and only slightly, if at all, in dogs, after lesion of the vagi. Besides, as I remarked in a review written several years ago,[1] "it could be readily shewn that this structural difference does not depend upon the circumstance of the muscle being called into contraction by the influence of volition, or in any other way, but upon the mode in which the contractility manifests itself; or to use more correct language, this structural difference in the muscular fibre causes a difference in its contractile properties, altogether unconnected with the circumstance of its entering into the formation of an organ of voluntary or of involuntary motion. Those muscles which can contract and relax rapidly, possess the former structure (striated); those which contract slowly and in a vermicular manner, present the latter structure (non-striated). As the first kind of contraction could be alone useful in an organ of voluntary motion, all the muscles placed under the influence of volition, possess the former structure; but as there are certain rapid muscular movements required in the involuntary or organic functions of the body, such as the contractions of the heart, and those of the pharynx in deglutition, all the muscles of organic life do not possess the latter structure. The muscular fibres of the pharynx present the striated appearance, and those of the heart also possess these striae, though somewhat indistinctly. It is more probable that the slight structural difference between the muscular fibres of the heart and those of a voluntary muscle are connected with the difference of the mode in which their contractile properties manifest themselves, than with the circumstance that the one acts involuntarily and the other voluntarily. If we could agree in designating that kind of contractility which manifests itself by rapid

[1] British and Foreign Medical Review. Vol. xiii., p. 164, 1842.

contractions and relaxations—*muscular contractility*, as occurring in the most characteristic parts of the muscular tissue; and that which manifests itself by slow contractions and relaxations, as in the stomach and intestines—*simple contractility;* we could then state that the muscles which possess the property of *muscular contractility*, present the striated appearance, while those which have the property of *simple contractility*, shew no longitudinal or transverse striae, and are smaller and more flattened. Whether this phraseology be adopted or not, it is obvious that the terms voluntary and organic muscles, used with a reference to these structural differences, may lead to error."

Cardiac Branches of the Vagus.—Various experimenters have removed portions of the vagus and sympathetic on both sides of the neck, without perceiving any effect upon the heart's action, beyond what could be sufficiently accounted for by the pain of the incisions, and the terror of the animal. In the dog and some other quadrupeds, these two nerves are so intimately associated in the neck that we cannot experiment upon one of them separately. Valentin[1] affirms that the division of the vagi in the neck increases the frequency, but diminishes the vigour of the contractions of the heart. We have seen no proof that the force of the heart is *directly* affected by this operation. This, however, could be satisfactorily determined by the use of the hemadynamometer, care being at the same time taken, that the free passage of air into and out of the lungs be not impeded. The attempts to ascertain if the movements of the heart can be influenced by excitation of the nerves leading to it, have yielded discordant results. There can, at least, be no doubt that the heart cannot be excited to contraction, by mechanical and chemical excitation of its nerves, so readily as the muscles of voluntary

[1] Opus cit., p. 48.

motion; in fact many experimenters have failed in affecting the contractions of the heart by these means. The sources of fallacy in judging of the relative strength and quickness of the heart's action, even within a short period, which cannot fail to be noticed by those who have watched the movements of this organ in animals immediately after death, are sufficiently great to excite distrust of the accuracy of the inferences of those who have stated that they have succeeded in this experiment.[1] Humboldt,[2] and Burdach have affirmed, that they renewed and increased the heart's contractions by the application of galvanism to the cardiac nerves; and Burdach[3] has stated, that he has quickened the movements of the heart of a rabbit, by applying caustic potass to the trunk of the sympathetic, or its inferior cervical ganglion. Brachet[4] supposes that the reason why the excision of the sympathetic ganglia in the neck does not arrest the heart's action is, that there is another source of nervous influence for the cardiac nerves placed below this, in the cardiac plexus or ganglion; and he assures us that the total destruction of the cardiac plexus is followed by the sudden and permanent arrestment of the heart's action; but this is an experiment which, from its nature, deserves no confidence. Valentin[5] believes that motor fibres are sent to the heart from the spinal accessory, and the three or four superior spinal cervical nerves, and reach this organ through the vagus and sympathetic; as he believes he succeeded in increasing the contraction of the heart by mechanical and chemical excitation of these nerves. Stilling[6] also supposes that he observed

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[1] Volkmann (Müller's Archiv. 1842, S. 372,) has related several experiments, illustrating these sources of fallacy.

[2] Ueber gereizte Muskel-und Nervenfasser. Bd. ii. S. 342-3, 1797, as quoted by Valentin.

[3] Traité de Physiologie, tom. vii., p. 74, traduit par Jourdan. 1837.

[4] Du Systême Nerveux Ganglionaire, Chap. Premier, § iv.

[5] Opus cit., pp. 49, 62, 65, 68.

[6] Häser's Archiv. iv. 1842.

increased contractions of the heart on irritating the vagus. Longet failed in influencing the rhythm of the heart by mechanical and galvanic excitation of the trunk of the vagus in the neck in dogs, rabbits, and sheep, but says that he was able, by scraping the cervical cardiac branches of the vagus in large dogs and in sheep, generally to produce this effect. Van Kempen[1] informs us that he has performed the experiment of attempting to influence the movement of the heart by excitation of the roots of the accessory upon five dogs and four rabbits, but obtained no satisfactory evidence of this. "No doubt," he says, "it is true the movements of the heart become occasionally increased in frequency after galvanic excitation of the roots of the accessory, but I cannot allow myself to draw any conclusion from this fact, since I have several times observed the same thing, especially in two rabbits, although I had not irritated the roots of the accessory. On one occasion even these movements recommenced spontaneously after having ceased for a short time." I have repeatedly applied galvanic excitation to the vagi and sympathetic, and like many others who have made this experiment, have not obtained any satisfactory evidence that it influenced the movements of the heart.

The effects of excitation of the vagi by the electro-galvanic rotatory apparatus are highly interesting. The Webers[2] state that excitation of both vagi by the electro-galvanic rotatory apparatus diminishes the frequency, and even arrests entirely the movements of the heart, reducing it to a state of complete repose, so that the blood flowing to it distends its cavities; and it resumes its movements on the withdrawal of the excitant. The same effects follow electro-magnetic excitation of the medulla oblongata. Si-

[1] Opus cit., p. 65.

[2] Wagner's Handwörterbuch. *Article* Muskelbewegung, S. 42 bis 47, already quoted.

milar experiments with the same results have been performed by Budge.[1] Electro-magnetic excitation of the medulla oblongata, though it throws the voluntary muscles into tetanic contraction, has no effect upon the heart when the vagi have been divided. Electro-magnetic excitation of the sympathetic nerve does not produce these effects upon the heart. The explanation of electro-magnetic excitation of the vagi and medulla oblongata upon the heart is still involved in doubt, but it seems to act upon the heart somewhat similarly to an extensive injury of the central organs of the nervous system, or of other parts of the body, producing a condition of the heart which forms the most important symptom in what surgical writers term *concussion*, and the *shock* from an injury or severe operation.

Effect of Lesion of one Vagus upon the Lung of that side.—This is a point of considerable importance in enabling us to judge whether lesion of the vagi acts directly or indirectly in inducing morbid changes in the lungs, for it is obvious, that if after the division of one vagus, the corresponding lung, and it only, underwent the same morbid changes as both lungs do after the division of both vagi, the inference would be that lesion of the vagi produces morbid action in the lungs directly, and not indirectly as we have maintained. Longet[2] describes various morbid changes as the result of the division of the vagus on one side. The corresponding lung, he says, is permeable up to the third day, but is emphysematous to a considerable extent, "une assez grande étendue." At the end of the seventh day it is less emphysematous, but in a considerable number of points it is manifestly engorged particularly at its summit. At the end of the second week, although the

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[1] Wagner's Handwörterbuch. *Article* Sympathischer Nerv mit besonderer Rücksicht auf Herzbewegung. Bd. iii., S. 410 bis 421. 1847.

[2] Opus cit., tom. ii., pp. 351, 352.

lung can be artificially inflated, yet it is evident that a considerable engorgement renders it less permeable than that of the opposite side. There are also in the bronchial tubes frothy mucosities in somewhat greater quantity than in the sound lung. At the end of the sixth week certain parts had ceased entirely to be permeable to the air and to blood, but some lobules were still easily distended by pushing air into the trachea. A section of the lung showed that it had partially lost its areolar structure, a red and frothy fluid flowed out of it, and the whole lung was generally of a deeper tint than the sound lung. Longet also refers to an experiment of Béclard and Descot upon a dog, which lived two months after the left vagus was divided. On opening the chest about four or five ounces of a purulent sanguineous serosity flowed from the cavity of the left pleura, the lung of that side was engorged with blood, and an abscess was present in the external surface of its upper lobe. Those who have described morbid changes occurring in one lung after section of the vagus of that side do not agree in their account of the nature of these changes, nor as to the period of their occurrence. It appears that Dupuytren[1] could detect no alteration in the lung on the side on which he had divided the vagus in two dogs and a horse, though these animals were allowed to live more than a month after the operation. Mendelssohn[2] has repeated this experiment on five rabbits, which were allowed to live from four to one hundred and eleven days—three of them living beyond twenty-eight days—and could detect no morbid change in the lung of the side on which the vagus was divided, though a portion of the nerve, to the extent of between four and six lines in length, was in each case removed.[3]

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[1] Biblioth. Médic. tom. xvii., p. 21. 1807.
[2] Der Mechanismus der Respiration, &c., S. 23, 24.
[3] Opus cit., p. 45.

The experiments of Dupuytren, one related by Magendie, those made by myself,[1] and those of Mendelssohn, are sufficient to prove that lesion of one vagus does not necessarily, or even generally, produce any morbid changes in the lung of that side.

Effects of Lesion of the Vagi upon the frequency of the Respiratory Muscular movements.—We may adduce the following additional evidence in proof of the fact that a considerable diminution of the respiratory muscular movements immediately follows lesion of the vagi. Arnold, in his experiments already several times referred to, observed, after section of the vagi, a considerable diminution in the frequency of the respiratory muscular movements. Traube in detailing his experiments upon rabbits, gives frequent illustration of this fact.[2] Mendelssohn, indeed, states[3] that this diminution of the frequency of the respiratory movements depends upon the paralysis of the muscles of the arytenoid cartilages, that it follows section of the recurrents even when the trunks of the vagi are uninjured, and that the respiratory movements become of the usual frequency when a tube is introduced into the trachea. The last part of this statement is at complete variance with the results of the experiments we have performed on dogs; and we have seen that there are equally little grounds for believing that section of the recurrents alone, diminishes the frequency of the respiratory movements.

How far are the Pulmonary Branches of the Vagus motor and sensiferous?—Dr. C. T. B. Williams failed in obtain-

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[1] My experiments were performed upon seventeen animals, which were allowed to live from twenty-four hours to six months after the operation.

[2] Die Ursachen und die Beschaffenheit derjenigen Veränderungen, welche das Lungenparenchym nach Durchschneidung der *nervi vagi* erleidet. *passim.* In Beiträge zur experimentallen Pathologie und Physiologie. Erstes Heft. Berlin, 1846.

[3] Opus cit., p. 61. Mendelssohn's experiments were performed on rabbits.

ing any evidence of contractions of the muscular fibres of the bronchial tubes by excitation of the vagi, but succeeded in effecting this by direct excitation of these muscular fibres, as by transmitting galvanism through the substance of the lungs.[1] Valentin observed contractile movements in the muscular fibres of the trachea, on irritating the recurrents in rabbits, and also in the horse, and from this infers that the pulmonary branches of the vagus may move the muscular fibres of the bronchial tubes.[2] Longet states that he succeeded in producing contraction of the muscular fibres of the bronchii by excitation of the vagi.[3] Volkmann, on repeating this experiment, failed in obtaining any evidence of the contraction of these muscular fibres by direct observation, but succeeded by the following method: He tied a tube, having a very fine orifice at one end, into the trachea—that end of the tube provided with the large orifice being within the trachea—and on bringing the flame of a candle to the external or fine orifice, and then irritating the vagi by galvanism, a sudden bending of the flame occurred.[4] As the muscular fibres of the bronchial tubes are endowed with that slow kind of contractility which unfits them from acting in unison with the muscles of respiration, we cannot conceive how they can aid in renewing the air within the lungs; but by diminishing the calibre of the bronchial tubes at particular parts, and thus causing the air to rush with greater force through these contracted parts, they may aid in expelling substances from the air-tubes, as in coughing.

Longet[5] concurs in the conclusion of Krimer and Brachet, that the sensibility of the mucous membrane of the lungs is destroyed by section of the vagi.

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[1] Transactions of British Scientific Association for 1840, p. 411.

[2] Opus cit., p. 50. [3] Opus cit., tom. ii., p. 289.

[4] Wagner's Handwörterbuch. *Article* Nervenphysiologie, S. 586.

[5] Opus cit., tom. ii., p. 289.

We have detailed several experiments to prove that the *besoin de respirer* is not annihilated by lesion of the vagi: and Volkmann,[1] Longet,[2] and Van Kempen,[3] have from their experiments arrived at the same conclusion.

We found that though the respirations were much diminished in frequency by the removal of the cerebrum and the cerebellum, and the division of the vagi, they nevertheless continued for a longer or shorter time; and similar results have also subsequently been obtained by Volkmann,[4] Flourens,[5] Longet,[6] and by Van Kempen.[7]

We have now detailed various experiments[8] to prove that though the vagi are the most important of the afferent or excitor nerves which convey those excitations to the medulla oblongata that produce the respiratory muscular movements, other cerebro-spinal nerves possess this property, though in a much feebler degree. We have also every reason to believe that those changes induced in the medulla oblongata itself by the circulation of venous blood along the arteries, and by the diminution or cessation of its usual supply of blood, are capable of exciting respiratory muscular movements. What are the excitations which call into action the involuntary movements of *expiration?* Do the same excitations that occasion the muscular movements of inspiration also cause the expiratory muscular movements which immediately follow? And are they to be regarded only as two separate stages of the same muscular action? We are disposed to answer these questions in the affirmative.

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[1] Müller's Archiv. Jahrgang 1841. S. 332.

[2] Opus cit., tom. ii., pp. 291-2. [3] Opus cit., p. 67.

[4] Müller's Archiv. Jahrgang 1840. S. 494. This, however, is not distinctly stated by Volkmann.

[5] Recherches Expérimentales sur les Propriétés et les Fonctions du Système Nerveux, &c. 2d Edit., p. 204. Paris, 1842.

[6] Opus cit., tom. ii., pp. 307-8. 1842. [7] Opus cit., p. 68. 1842.

[8] *Vide* pp. 185-6 of this volume.

Longet distrusts the inferences of Dr. M. Hall and Mr. Broughton, that respiratory movements may be induced by excitation of the trunk of the vagus, and regards the effects witnessed by these experimenters merely as indications of pain; but we have already stated that we have observed these in animals deprived of volition. Romberg believes he produced the reflex muscular movements of coughing in a horse by excitation of the trunk of the vagus.[1]

Morbid Changes in the Lungs.—The late researches of Valentin, Longet, Mendelssohn, and Traube, bear out the general description of the morbid changes in the lungs, after division of the vagi which we have given, viz., engorgement of a greater or smaller part of the lungs with blood, a variable quantity of frothy serum frequently tinged with blood in the bronchial tubes, and not unfrequently condensation of portions of the lungs from the effusion of the solid constituents of the blood, or from inflammatory effusions, with more or less emphysema of other portions. Mendelssohn[2] attributes these morbid changes entirely to the paralysis of the recurrent nerves impeding the free passage of air into the lungs, and he maintains that lesion of the recurrents alone, without injury of the trunks of the vagi, causes the same morbid changes in the lungs as the lesion of the trunks of the vagi. These morbid changes can also, according to him,[3] be produced by making an opening into the cavity of the pleura, by injecting a quantity of adhesive fluid into the trachea, by placing foreign bodies within the trachea, and, in fact, by any means which diminish to a notable extent the passage of a quantity of air into the lungs necessary for carrying on effectually the function of respiration. Traube,[4] on the other hand, after disproving the explanation given by Mendelssohn of the

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[1] Müller's Archiv. 1838. S. 311.

[2] Opus cit.

[3] Opus cit., p. 79.

[4] Opus cit.

cause of these morbid changes in the lungs from lesion of the vagi, endeavours to show that they originate in the paralysis of the intrinsic muscles of the larynx permitting the secreted fluids poured into the mouth to pass down the trachea and impede the free æration of the blood. He states that he satisfied himself by the microscope, of the presence of pavement, epithelium cells, and other constituents of the saliva, in the fluids taken from the trachea and bronchial tubes.[1] It is quite true that animals after lesion of the vagi, may die in consequence of the paralysis of the muscles attached to the arytenoid cartilages, in a manner explained at considerable length in the preceding pages (pp. 102-115); and that substances pass readily in the rabbit[2] through the larynx into the air-tubes, after lesion of the vagi, and may hasten the death of the animal. That animals will die in the usual manner, after lesion of the vagi, when means have been taken to secure the free passage of fresh air into the lungs, and when none of the contents of the mouth or stomach can pass into the bronchial tubes, as can be done by inserting a large tube into the trachea and securing it firmly there, has been fully proved by the experiments we have performed. It must also be remembered that dogs can swallow large quantities of liquid substances, as we have often witnessed, after section of both vagi and recurrents, without any of them passing into the air-passages. I see no reason to depart from the explanation of the cause of death after lesion of the vagi, which I formerly gave,[3] viz., that it depends upon the great diminution in the number of the respiratory

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[1] Opus cit., p. 130.

[2] The whole of Mendelssohn's, and nearly the whole of Traube's experiments, were performed on rabbits. All the experiments from which Traube draws his conclusions regarding the cause of death after section of the vagi, were performed on these animals.

[3] *Vide* p. 204 of this volume.

movements. This explanation has been adopted by Volkmann.[1]

Effects of Lesion of the Vagi on the Stomach.—Longet has, from his experiments, arrived at conclusions similar to ours upon the effects of lesion of the vagi upon the sensation of hunger.[2]

Arnold[3] ascertained, in his experiments upon hens and pigeons, that the gastric juice secreted after section of the vagi was acid, that it was not perceptibly diminished in quantity, and that it was capable of converting the food into chyme. Longet found that section of the vagi in quadrupeds neither arrests the secretion nor the acidity of the gastric juice, and believes that an explanation entirely mechanical, will account for the slight diminution in its quantity.[4]

Several late experimenters, among whom we may enumerate Valentin,[5] Bischoff,[6] and Longet, have succeeded in producing vermicular contractions of the muscular fibres of the stomach, by mechanical and galvanic excitation of the trunk of the vagus; and the Webers and Budge in their experiments with the electro-galvanic rotatory apparatus witnessed the same effects.[7] Volkmann[8] and Van

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[1] *Article* Nervenphysiologie in Wagner's Handwörterbuch, &c., pp. 587-8. 1845.

[2] Opus cit., tom. ii., p. 329.

[3] Opus cit., 142.

[4] I perceive that in the article *Par Vagum* in the Cyclopædia of Anatomy and Physiology, it is inadvertently stated that Longet believed that the secretion of gastric juice was even increased after section of the vagi. Several errors in that article would have been avoided if an opportunity of correcting the press had been afforded me.

[5] Opus cit., p. 52.

[6] Müller's Archiv. 1838. Note at p. 496.

[7] *Vide* the *articles* in Wagner's Handwörterbuch already quoted.

[8] Müller's Archiv. 1840. S. 496. Volkmann, however, mentions that he saw Bischoff perform this experiment upon a dog, and the result was to satisfy him that in that case the muscular fibres of the stomach were thrown into contraction by mechanical excitation of the vagus. *Article* Nervenphysiologie in Wagner's Handwörterbuch, p. 585.

Kempen,[1] on the other hand, failed in producing muscular contractions in the stomach, by mechanical and galvanic excitation of the root of the vagus. It would appear that this experiment succeeds best on dogs, and when the stomach contains some food.

Spinal Accessory Nerve.—We have already given the results of the recent experiments upon the roots of the accessory, made with the view of ascertaining the functions of its internal branch. In extension of these, I have to refer to the repetition of the experiment of Bischoff upon the effects of the destruction of the roots of the spinal accessory upon the voice,[2] by Longet, Morganti, and Bernard.

Longet informs us that in six attempts upon four dogs and two goats, to repeat the experiment of Bischoff, he failed, as the animals died from hemorrhage before its completion; but in the seventh upon a dog, where the roots of the nerve were completely destroyed on one side, and in a great measure on the other, the movements of the muscles of the larynx were arrested on the side on which the nerve was destroyed, and they were much enfeebled on the other side.[3] Morganti states that a dog in which he cut the trunks of the accessory nerves lost its voice; and he observed that the movements of the vocal cords were arrested on the side on which the roots of the accessory had been divided.[4] Bernard, who experimented by destroying the roots of the accessory through the foramen lacerum posterius, without opening the spinal canal or cranium, states, that he succeeded in destroying the roots of the spinal accessory nerves on both sides in several animals, (cats, rabbits, and dogs,) and found that they had lost their

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[1] Opus cit., pp. 35 and 81.

[2] *Vide* p. 153 of this volume.

[3] Opus cit., p. 263; also Recherches Expérimentales sur les fonctions des nerfs, des muscles du larynx, &c., p. 30. 1841.

[4] Omodei, Annali Universali di Medicina. Juli 1843.

voice, had some difficulty in swallowing, and were more easily blown than usual on making an exertion. On examining the larynx it was open, and the animals could not shut it. The functions of respiration, circulation, and digestion in those animals were unaffected.[1] Bernard maintains that although respiration and phonation seem anatomically confounded, since they are performed by the same apparatus, these two functions are nevertheless physiologically independent, and that while the vagus acts in producing the muscular movements of the former of these functions, the spinal accessory regulates the muscular movements of the larynx and chest engaged in the latter function. The spinal accessory is then, according to him, a motor nerve which regulates solely the movements of the larynx and thorax, every time that these organs produce *phonation*, and is consequently engaged in the production of actions which are beyond the aim of simple respiration. Bernard observed as we had previously done,[2] that division of the external branch of the spinal accessory does not prevent the muscles in which it is distributed from acting as muscles of respiration; but as in his experiments the animals might have used these muscles *voluntarily*, they are liable to a source of fallacy against which we made provision.]

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[1] Archives Générales de Médecine, tom. v. 4th série, pp. 59 to 65. 1844.
[2] Opus cit., tom. iv., pp. 404, 405, and tom. v., p. 86.

No. VI.

ON THE EFFECTS OF LESION OF THE TRUNK OF THE GANGLIONIC SYSTEM OF NERVES IN THE NECK UPON THE EYEBALL AND ITS APPENDAGES.

(FROM THE EDINBURGH MEDICAL AND SURGICAL JOURNAL, AUGUST, 1839.)

In a former communication in this Journal (for January 1838, p. 132[1]) I stated that I had frequently verified the observations of Petit and others, that when the *vagus* is injured in the neck in those animals in which the sympathetic is combined with the *vagus*, as in the dog—that the *conjunctiva* becomes inflamed, the pupil contracts, and the eyelids are somewhat more closely approximated to each other. At that time I suggested that the contracted pupil and partially closed eyelids might probably depend upon the impatience of light which frequently accompanies inflammation of the *conjunctiva*. I have since attended more carefully to this subject, and during the last summer satisfied myself that the contraction of the pupil, the projection of the cartilaginous membrane or third eyelid, situated at the inner angle of the eye, over the cornea, and the partial approximation of the eyelids to each other, take place immediately after the injury of the sympathetic, and before the inflammation of the *conjunctiva* presents itself, and that

[1] *Vide* pages 95 and 96 of this volume.

they continue after it has disappeared. I shall here detail a few of the experiments I have made in elucidation of this point.

1*st Exper.*—The *nervus vagus* and sympathetic were cut on the left side in a small terrier, and instantly the pupil of the eye became considerably diminished in size, and the cartilaginous membrane at the inner angle of the eye was forced over the internal margin of the *cornea*. A quarter of an hour after the division of the nerves, the *conjunctiva* of the left eye appeared more vascular than that of the right, and the *cornea* was perhaps a little dimmer. There was also some increased secretion of tears from the left eye, and the condition of the pupil and third eyelid remained as before.—Twenty-four hours. The *conjunctiva* of left eye was vascular and covered with a thick tenacious mucus, and the appearance of the *iris* and the third eyelid was the same as immediately after the division of the nerves. Some slight vascularity of *conjunctiva* of right eye, and some increased secretion of mucus, but there was no apparent change upon the pupil and third eyelid.—Third day. The *cornea* of left eye was dim, and the *conjunctiva* very vascular, and covered with much tenacious mucus. The *conjunctiva* of right eye was somewhat vascular, and covered with a small quantity of mucus.—Seventh day. The *cornea* of left eye was less dim, and there was less inflammation of *conjunctiva*, but it was still covered by a considerable quantity of puriform mucus. The pupil was still contracted, and the cartilaginous membrane projected over the *cornea*. The animal was now made the subject of another experiment. In this and in several other experiments which I have made with a similar view, I was assisted by my friend Dr. Staberoh of Berlin.

2*d Exper.*—The *vagus* and sympathetic were cut across on one side, and a portion removed in a middle-sized cocker dog, after the exact similarity of the two eyes had been

ascertained. Immediately after the section of the nerves, the pupil became contracted, the third eyelid projected over the inner edge of the *cornea*, the eyeball was apparently placed deeper in the socket, and rolled inwards, and the eyelids partially closed. At this time there was no perceptible redness of the *conjunctiva.* Ten minutes after the section of the nerves the pupil and eyeballs remained in the same state, and there was no vascularity of the *conjunctiva.*—Twenty-four hours. The *conjunctiva* of the side operated upon was very vascular, with increased secretion of tears; the *cornea* was, however, clear, and there was no inflammation observable in the deeper parts of the eyeball. The opposite eye had undergone no change from the natural condition.—Forty-eight hours. No perceptible change since yesterday.—Fifth day. Rather less vascularity of the *conjunctiva* of the side operated upon, and it was partially covered with some puriform mucus. The other appearances remained as before.—Sixth day. Less vascularity of *conjunctiva*, no other perceptible change.—Eighth day. Vascularity of *conjunctiva* nearly gone, the pupil is apparently less contracted, and the third eyelid projects decidedly less over the *cornea.*—Tenth day. There are still some remains of conjunctival inflammation. The pupil is still very perceptibly smaller than that of the sound eye; the cartilaginous membrane projects less over the *cornea*, and the eyelids are less approximated, though evidently closer to each other than in the sound eye.—Third week. Scarcely any traces of redness on the *conjunctiva;* the third eyelid still projects forwards, but it does not now encroach on the *cornea;* other appearances remain as before.—Five weeks. Redness of *conjunctiva* entirely gone, but other appearances remain unchanged.—Two months. The pupil of the eye of the side operated on is still very decidedly less than that of the opposite side, and the eyelids are evidently somewhat more approximated. The pupil was not, however, motionless,

but it has continued to contract and dilate when exposed to a stronger or feebler light. The animal was now killed by a dose of prussic acid, and it was observed that while expiring, the pupils of both eyes were much dilated, and became of equal size.

3d *Exper.*—The left superior cervical ganglion of the sympathetic was removed in a dog in which the common carotid of that side had been previously secured to prevent hemorrhage. Great care was taken to avoid injury of the *nervus vagus*. The lower half of the ganglion was at first only removed, and this was immediately followed by contraction of the pupil, the projection of the third eyelid over the inner edge of the *cornea*, and the other appearances remarked in the previous experiment. One minute after this the whole of ganglion was removed without any apparent increase of the effects previously produced.—Twenty-four hours. Scarcely any increased redness of left *conjunctiva*, but there is some slight increased secretion of mucus. Pupil as yesterday. The eye remained nearly in the same state during the fortnight it was allowed to live. It ate freely, was quite active, and never showed any tendency to stupor.

In the dog, as I have already remarked, it is impossible to cut the *nervus vagus* in the middle of the neck without also dividing the trunk of the sympathetic. In the cat, however, though these two nerves lie in the same sheath, yet by a little care they can be easily separated from each other opposite the thyroid cartilage; and in the rabbit they have nearly the same relation to each other as in the human species. The cat and the rabbit consequently furnish an opportunity of experimenting upon these nerves singly.

4*th Exper.*—The sheath of the right carotid was exposed high in the neck in a kitten, and the sympathetic was then cautiously separated from the *vagus* without injuring the latter. When the sympathetic was compressed with a moderate force, the right pupil began to contract gradually,

and became much smaller than that of the left eye; and it again resumed its former size on removing the pressure.[1] A portion of the right sympathetic was now removed, and the pupil again contracted slowly, and remained permanently smaller than that of the left eye; the cartilaginous membrane was pushed considerably over the anterior surface of the cornea, and the eyelids were partially approximated to each other.—Twenty-four hours. No distinct redness of *conjunctiva*. Condition of right eye the same as yesterday, and the pupil, though constantly smaller than the left, contracts and dilates within certain limits and exposure to a stronger or feebler light. The animal was lively. It died seven days after the division of the nerve. At the time of its death no distinct redness of the *conjunctiva* had presented itself, and the eye had the same appearance as the day after the operation.

5th Exper.—The left sympathetic was cautiously separated from the *vagus* in the neck of a full-grown cat, and cut across. The iris immediately contracted slowly, and gradually, and soon presented a marked contrast to the right pupil, and the third eyelid was pushed over the inner surface of the *cornea*. The right vagus was now exposed, separated from the sympathetic, and divided without injuring the latter. This was followed by no change upon the right eye. The right sympathetic was now divided, and the same phenomena presented themselves as in the left eye. Dr. Alison was present at this experiment. No distinct redness afterwards presented itself in the *conjunctiva* of either eye. When the animal was killed three weeks after division of the nerves, the cartilaginous membranes at the inner angles of the eye, though more distinctly visible

[1] This experiment of producing contraction of the pupil by compressing the trunk of the sympathetic in the neck, I have repeated with success in other cases not detailed here.

than usual, did not project over the *cornea;* the pupils were exactly similar, and appeared to have nearly recovered their usual size. As in this experiment, however, both sympathetics had been cut, we could not judge of the effects of the operation upon the pupil as in the cases where the sympathetic was divided on one side only, for then the sound eye served as a standard of comparison.

6*th Exper.*—I removed a portion of the right sympathetic from the neck of a full-grown cat in the presence of Dr. Monro and Mr. Mackenzie. On cutting the nerve across the pupil was instantly seen to contract slowly and gradually, and soon presented a marked contrast to that of the opposite side. The cartilaginous membrane at the same time gradually encroached upon the surface of the inner part of the *cornea*, the eyeball appeared deeper, and the eyelids more approximated than those of the opposite eye. A month after section of the nerve, the size of the pupil still presented a very striking difference from that of the left eye; and the cartilaginous membrane projected more than that of the opposite side, though it no longer encroached upon the *cornea*. The animal never lost its activity.[1]

7*th Exper.*—In one rabbit the trunk of the sympathetic was first divided on one side of the neck, and a portion removed; and a few days after, the same operation was repeated on the opposite side. In other seven rabbits the superior ganglion of the sympathetic or a large portion of the trunk of the nerve, was removed on one side without tying or injuring any of the large blood-vessels or any other nerve, and in two of these the same operation was repeated on the opposite side. In one of these only was there any

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[1] In a cat on which I assisted Mr. Little to repeat this experiment, the pupil was nearly natural a month after portions of the sympathetic and *par vagum* on one side were removed.

change observed upon the iris, and no decided increased redness of the *conjunctiva* presented itself. In one of these animals it was remarked that the eyelids of the side on which the superior ganglion of the sympathetic had been removed, were less apart than on the opposite eye; but whether this was the effect of the removal of the ganglion, or of some slight injury received during the operation, we cannot at present pretend to determine. From these experiments it would appear that in rabbits the superior ganglion of the sympathetic, and a considerable portion of the trunk of that nerve as it lies in the neck, may be generally removed without effecting any change upon the iris; while the compression or section of the trunk of the sympathetic in the neck in dogs and cats is instantly followed by contraction of the pupil, the forcing of the cartilaginous membrane over the inner part of the anterior surface of the eyeball, the retraction of the eyeball deeper into the socket, and a slight approximation of the eyelids. In dogs this also is followed—sometimes after a very few minutes, but generally after a longer interval—by inflammation of the *conjunctiva*, which is occasionally so severe that this membrane presents an almost uniform redness, and is covered by puriform mucus, and the cornea becomes dim. As far as I have been able to observe this, inflammation is confined to the *conjunctiva*. To judge from the limited number of experiments I have yet made upon cats and rabbits, the inflammation of the *conjunctiva* in the former is trifling, if present at all; and in the latter it is entirely absent. I was at first inclined to believe that the outward projection of the third eyelid—for in the dog and cat it has no muscles attached to it, was dependent upon the rolling inwards of the eyeball; but subsequent observations have nearly satisfied me that this depends upon the *retrahens oculi* muscle drawing the eyeball deeper into the orbit, by which the fat is pressed forwards, and the third

eyelid pushed over the anterior surface of the eyeball. This would also explain the approximation of the eyelids. I find it impossible at present to give any thing like a plausible explanation of the effects of injury of the sympathetic upon the eyeball and its appendages, and the cause of their dissimilarity in different animals. It is evident, however, that this is to be sought for in the connexion of the branches of the sympathetic with the encephalic nerves of the orbit, and especially with the sixth pair, and those branches forming the ciliary nerves. I intend at my earliest opportunity to endeavour, by extensive minute dissections of the ascending branches of the superior sympathetic ganglion in various mammalia, to give some probable solution of this question. We may then be able to judge whether an injury of the cervical portion of the sympathetic in man, such as may possibly occur in certain diseases and operations on the neck, would be followed by contractions of the iris and inflammation of the *conjunctiva*. In a case described in the Medical Gazette,[1] where the right carotid, the vagus and surrounding parts are described as being enveloped in a large morbid tumour, and where, consequently, the sympathetic could hardly be supposed to escape, the pupil of that side is described as becoming smaller during the course of the disease.

This contracted state of the pupil, consequent upon lesion of the sympathetic in the neck, is not noticed in the experiments of Cruickshank,[2] Arnemann,[3] Mayer of Bonn,[4] and

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[1] September 29, 1838, p. 16, vol. xxiii.

[2] Medical Facts and Observations, vol. vii., p. 136, or Phil. Trans. 1795, Part I.

[3] Versuche über die Regeneration der Nerven, S. 69, 85-6-7-9, 94-6-7-9, 102. Göttingen, 1787.

[4] Journal der Chirurgie von Graefe und Walther, Zehnter Band, S. 418. 1827. Mayer's experiments upon the effects of lesion of the vagus and sympathetic were almost entirely performed on rabbits. His eleventh ex-

Brachet,[1] though all these authors describe its effects upon the *conjunctiva.* Indeed, Arnemann expressly states, " that he has not observed the changes upon the pupil which Molinelli has remarked after ligature of the eighth nerve ;"[2] and Brachet, in relating his first experiment, where the trunk of the sympathetic was divided in the neck of the dog for the purpose of observing its effects on the eye, makes the following observation :—" L'iris n'en a reçu aucune influence marquée ; il a continué de se contracter suivant la vivacité de la lumière : cette remarque ayant été commune à toutes les expériences, je ne la reproduirai pas." Petit[3] remarked this contraction of the pupil in some of his experiments ; but his observations would lead us to believe that it was a subsequent and not an immediate effect, and could not enable us to decide whether or not it was dependent upon the inflammation of the *conjunctiva.* In the 1st Experiment of the second series of his two sets of experiments,[4] the right vagus was cut on the 18th September ; and, on the 19th, he remarked, among other changes on the eye of that side, that the pupil was less. The left vagus of the same animal was afterwards divided, (2d Experiment, p. 11,) and in a quarter of an hour he observed the cornea flattened, and the pupil contracted. In the 3d and 4th Experiments, he observed the pupil to be contracted one hour after section of the nerves. In the 5th Experiment he made this curious observation, that when the

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periment is, however, detailed as follows :—" An einem Esel, einem Hunde, und Kaninchen, wurde der Nervus vagus unterbunden. Man bemerkte keine Veränderung am Auge."

[1] Recherches Expérimentales sur les Fonctions du Système Nerveux Ganglionaire, Chap. ix., Expér. 150-1-2, &c.

[2] Oper. cit., s. 96.

[3] Mémoire dans lequel il est démontré que les Nerfs Intercostaux fournissent des rameaux qui portent des esprits dans les yeux. Histoire de l'Académie Royale, 1727.

[4] Oper. cit., p. 10.

nerves were cut on both sides, dilatation of the pupil followed, and more in the right eye than in the left. This animal, it is worthy of remark, suffered much from dyspnœa, and died in twelve hours. In the numerous experiments I have made upon the effects of section of the vagi, I never observed any thing similar to this; on the contrary, the pupils always became contracted.

Molinelli[1] relates five experiments upon dogs, in which he watched the effects of ligature of the vagus and sympathetic upon the eye. In one experiment he informs us, (p. 281,) that a little after the left vagus was tied, the *conjunctiva* of the left eye became red, and the cartilaginous membrane at the inner angle of the eye projected over the cornea. On the seventeenth day after the operation, he observed that the pupil of that eye was diminished in size. In the 3d Experiment, (p. 284,) the left vagus was tied with a double ligature on the 14th January; and on the 30th it was remarked that the pupil of the left eye was twice as small as the right, and that the eyeball seemed depressed. In the 4th Experiment, he mentions a change of colour in the iris, and there is nothing said about the diminution of the pupil, but on the 22d day after the operation, he states that it had returned to its former condition. Dupuy[2] in two of his experiments mentions this contraction of the pupil. In one experiment (Oper cit. Premier Fait, p. 343,) he remarked that the pupil became contracted immediately after the superior ganglion of the sympathetic had been extirpated. His words are these,—" Aussitôt après l'opération, l'œil de ce côté parut plus enfoncé dans l'orbite, les paupières étaient tumifiées, la membrane clignotante se portait en avant du

[1] Comment. Bononiensi, tom. iii., 1755, p. 280.

[2] Journal de Médecine, Chirurgie, &c., Décembre 1816, tom. xxxvii., p. 340.

globe oculaire, la pupille se resserra." In a subsequent experiment, (Troisième Fait, p. 347,) the operation was performed on the 26th April; and on the 10th May he observed that the pupil was contracted. Though I have occasionally observed very severe inflammation of the *conjunctiva* follow this operation in dogs, yet I have never seen it proceed to the disorganization of the eyeball. In one of Arnemann's experiments it appears to have produced ulceration of the cornea.[1] In two of Mayer's experiments upon rabbits, where the vagus and sympathetic were not only enclosed in ligatures, but the common carotid was also tied, inflammation of the cornea occurred, and this in one case was followed by ulceration, and in the other by staphyloma, and the effusion of a layer of lymph upon the anterior surface of the iris, obliterating the pupil.[2] Brachet relates several experiments to show that injury of the sympathetic or destruction of its superior cervical ganglion is attended by great vascular congestion of the anterior and middle lobes of the brain, producing drowsiness and stupor.[3] The experiments which I have made on this point do not by any means confirm those of M. Brachet. As the fact, if correct, is one of great importance, and ought to be very carefully investigated, I shall reserve the consideration of this and some of the other questions connected with the lesion of the cervical portion of the sympathetic, and more especially the length of time the contracted state of the iris continues after a portion of the nerve has been removed, until a future opportunity.

[Since the above remarks were published, I have been made acquainted through Stilling's Treatise on Spinal

[1] Oper. cit., Acht-und-sechzigster Versuch, S. 69.
[2] Oper. cit., Experiments 17, 18.
[3] Oper. cit., Experiments 155 to 159, and 160.

Irritation,[1] with the results of the experiments of Camerer,[2] and Pommer,[3] upon the effects of lesion of the sympathetic nerve in the neck on the eyeball and its appendages. In their experiments, as in those we have detailed above, no change was observed upon the eyeball in the rabbit after section of the sympathetic and the vagus in the neck. Arnold in his experiments on hens and pigeons, observed no change upon the eyeball or its appendages on dividing the vagus, and as in these animals the trunk of the sympathetic nerve in the neck is not bound up with the vagus, he concludes that the effects upon the eye which follow lesion of the vagus in the dog, depend upon injury of the sympathetic and not of the vagus,[4] a conclusion which may be satisfactorily established by direct experiment on the cat, as we have shewn. (*Vide* 5th Experiment.) Longet after remarking that he had observed contraction of the pupil in the dog on cutting the vagus, goes on to say, as I understand the passage, that the vagus and sympathetic can be easily separated in the neck from each other in the rabbit, and that he never failed to observe this effect upon the pupil on cutting the latter nerve.[5] Valentin has investigated this subject extensively and carefully.[6] He has observed the effects upon the eyeball and its appendages from section of the conjoined trunk of the vagus and sympathetic in the dog, described above; and he agrees with Arnold that the diminution of the size of the pupil con-

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[1] Physiologische, pathologische und medicinisch-practische Untersuchungen über die Spinal Irritation. S. 139, 140. Leipzig, 1840.

[2] Versuche über die Natur der krankhaften Magenerweichung. 1828.

[3] Beiträge zur Natur-und Heilkunde. 1831.

[4] Bemerkungen über den Bau des Hirns und Rückenmarks. S. 123. 1838.

[5] Anatomie et Physiologie du Système Nerveux, &c., tom. ii., p. 353. Paris, 1842.

[6] De Functionibus Nervorum Cerebralium et Nervi Sympathici, pp. 109, 114. Bernae, 1839.

tinues until the regeneration of the divided portion of the nerve has taken place. He never observed any decided change upon the iris on dividing either the sympathetic or the vagus, or both of these nerves, in the middle of the neck in rabbits.[1] On removing, however, the whole of the ganglion of the trunk of the vagus, (ganglion inferius,) the pupil becomes notably altered, oblong, and often angular, especially at the upper edge.[2] On removing the superior cervical ganglion of the sympathetic, the pupil is still farther reduced in size, becomes longitudinally oblong, angular at the lower edge, and rounded above. This experiment succeeds upon an animal immediately after death, if it be performed before the tissues have lost their excitability. On applying a ligature upon the *nervi molles* and the internal carotid filaments that come from the superior cervical ganglion of the sympathetic, the same effects were produced upon the iris, as by removal of the ganglion itself. Along with these changes upon the iris, the eyeball was rolled more inwards than in the normal condition of the organ. From these and other facts, Valentin concludes that the iris derives its motor filaments from two sources,—from cerebral and from spinal nerves. Its cerebral motor filaments come from the inferior branch of the motor oculi nerve; its spinal motor filaments from the spinal cervical nerves. These latter in the rabbit come from the superior cervical nerves, and enter, on their passage upwards, the superior cervical ganglion of the sympathetic and the ganglion of the trunk of the vagus, (ganglion inferius.) In the dog, on the other hand, they come also from the lower spinal cervical nerves, and join the united trunk of the vagus and sympathetic in the lower

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[1] Opus cit., p. 110.

[2] It is necessary to remove the whole of this ganglion: removal of the lower half of the ganglion has no effect upon the pupil.

part of the neck. The cerebral filaments move the circular muscular fibres of the iris or the contractor muscle of the pupil; the spinal filaments move the radiating muscular fibres of the iris or the dilator muscle of the pupil. When therefore the spinal motor filaments are cut in the neck, the dilator muscle is paralyzed, while the contractor muscle, no longer antagonized by the extensor muscle, is permanently contracted, and the pupil is consequently diminished in size.[1] He infers from evidence which he is aware is far from being satisfactory, that the arrangement of the spinal motor filaments of the iris in the human species, resembles that in the rabbit. Valentin likewise believes that this view of the motor nerves of the iris, being derived from two different sources, and supplying antagonist muscles, will not only explain the effects of lesion of the sympathetic and vagus in the neck upon the iris, but enable us to understand the variety in the condition of the pupil as to contraction and dilatation in certain diseases, which have hitherto puzzled medical men; and also clear up some anatomical anomalies in the origin of the ciliary nerves, which have been recorded.]

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[1] Valentin found that rabbits of a black or black and white colour are better fitted than those of a white colour for the experiments here detailed, as the iris is more irritable in the former than in the latter. (Opus. cit., p. 110.)

No. VII.

ON SOME POINTS IN THE ANATOMY OF THE MEDULLA OBLONGATA.

(FROM THE EDINBURGH MEDICAL AND SURGICAL JOURNAL, JANUARY 1841.)

SINCE the brilliant discoveries of Sir Charles Bell have disclosed to us that the motor nerves are attached to the anterior portion, and the sensiferous nerves to the posterior portion of the spinal cord, the anatomical relations of the different tracts of this division of the central organs of the nervous system have assumed an interest and importance to which they had hitherto been a stranger. If we adopt, however, any of the views of the anatomical relations of the different tracts of the spinal cord, as they pass through the *medulla oblongata*, which are given in the anatomical works of this country, there appears to be some blending of the motor and sensitive tracts at that part which cannot be explained; for, while the hypoglossal and abducens nerves arise in the line of the anterior roots of the spinal nerves, we find other motor nerves, such as the *portio dura*, the motor part of the fifth pair, the trochleator, some of the filaments of the *par vagum*, and the spinal accessory, all arising from points posterior to this, and more in the line of the origin of the posterior than of the anterior roots of the spinal nerves.

The following remarks contain an attempt, founded on

repeated anatomical examination, to solve this difficulty. In speaking of the different columns or tracts of the spinal cord, we shall first consider each symmetrical half as consisting of four different columns. There are, 1. the *pyramidal* column, or that connected with the pyramidal body; 2. the *olivary* column, or that connected with the olivary body; 3. the *restiform* column, or that column occupying the lateral surface of the spinal cord between the lateral groove to which the anterior and posterior roots of the spinal nerves are attached, and after forming the greater part of the restiform body, proceeds upward through the *pons Varolii* to the *crus cerebri;* 4. the *cerebellar* or *posterior* column placed between the posterior lateral groove to which the posterior roots of the spinal nerves are attached, and the posterior longitudinal groove, and which forms the posterior pyramidal body, and posterior part of the restiform body, and passes entirely into the *crus cerebelli.* We shall also speak of each symmetrical half of the spinal cord, as being divided into three columns, as this is a division frequently adopted. Under the *anterior* column, we shall include both the pyramidal and olivary columns; the restiform will be the *middle* or *lateral* column; and the cerebellar will be the *posterior* column. Though the position of the anterior and posterior peaks of the cineritious matter of the cord may serve as a good anatomical division of the spinal cord through a great part of its extent, the anterior peak corresponding to the attachment of the anterior roots and the posterior peak to the posterior roots of the spinal nerves, yet, as we approach the *medulla oblongata,* it begins to fail us, and we there find the anterior peaks of the grey matter project decidedly in front of the origin of the anterior roots of the spinal nerves.

On tracing the pyramidal columns from the lower margin of the *pons Varolii* downwards to the well known *decussation* at the lower end of the pyramidal bodies, the greater part

of the fibres composing these columns arrange themselves into two or more bundles, which decussate or cross each other. These decussating fibres cross the bottom of the anterior longitudinal fissure, and proceed downwards and backwards to join the posterior part of the restiform or middle columns, which they accompany in their course downwards.

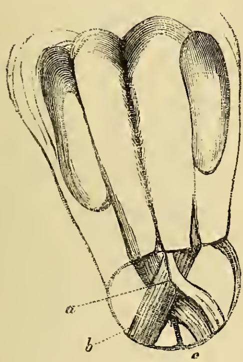

Fig. 1.[1]

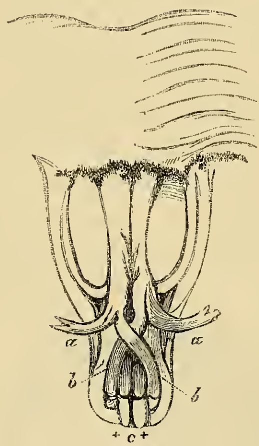

Fig. 2.[2]

Every one may readily satisfy himself that none of these decussating fibres proceed into the anterior column of the opposite side, but follow the course we have mentioned, by making a slightly oblique division of the spinal cord at the point of decussation, or by tearing down the pyramidal columns in brains rendered firm by immersion in alcohol, and carefully observing the course which they take. While the greater part of the fibres of the pyramidal columns decussate in the manner we have mentioned, a much

[1] Fig. 1. *Oblique section of spinal cord at the decussation of the pyramidal columns.* *a* Decussating fibres. *b* Middle column. *c* Posterior column.

[2] Fig. 2. *Anterior view of decussation.* *a* External fibres of pyramidal column detached and thrown outwards. *b* Decussating fibres running into posterior part of middle column. *c* Posterior columns.

smaller number of the external fibres of each column do not decussate, but proceed downwards on the same side, and, along with the olivary column, form the anterior column of that side of the spinal cord. In a small number of the numerous dissections we have made, the whole of the pyramidal column appeared to pass into the middle column of the opposite side. A band of the fibres of the pyramidal column, which in some cases are very distinct, curve round the lower part of the olivary body of the same side, and then proceed upwards and backwards to join the posterior or cerebellar column, as it passes into the *crus cerebelli*, (Fig. 3.) This band of fibres, figured by Santorini, and Gall, and Spurzheim, described by Rosenthal,[1] Rolando, and more lately by Mr. Solly, has been termed the *arciform* filaments or band. The filaments forming this arciform band do not decussate. In a preparation in my possession, (Fig. 3,) in which this arciform band is unusually strong, it is distinctly observed to divide itself into two parts, one of these, the larger, follows its usual course to join the posterior column, the other throws itself into the olivary column, (Fig. 3, *b*,) and proceeds downwards along with it.

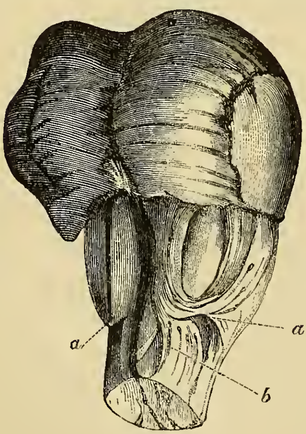

Fig. 3.[2]

On tracing the olivary columns downwards, they are observed to converge at the

[1] Ein Beitrag zur Encephalatomie, S. 28. Rosenthal describes this as a marked medullary layer passing from the pyramidal bodies over the inferior parts of the olivary bodies to the *posterior column*, and thus forming a strong bond of union to these parts, which varies in strength and thickness in different cases.

[2] Fig. 3. *a a* Arciform filaments of medulla oblongata.

lower part of the pyramidal bodies, and below the decussation they are only separated from each other by the longitudinal fissure, and the small band of the pyramidal column which proceeds downwards without decussation, (Fig. 4.) At the upper portion of the *medulla oblongata*, the olivary bodies are thus thrown apart by the thick bands of which the pyramidal columns consist at that part; while below the decussation the greater part of the pyramidal bodies have now passed backwards to join the middle or restiform columns, and the olivary columns are consequently thrown forwards, and compose the greater part of the anterior columns. On tracing the olivary column upwards, we observe that, after having surrounded the olivary body which lies among its fibres like a kernel, it passes into the *pons Varolii*, and there divides itself into two bands, which diverge from each other, one of them proceeding upwards and forwards to join the *crus cerebri*, the other proceeding upwards and backwards to reach the *corpora quadrigemina* or optic lobes,[2] (Fig. 6.) On scraping off the cineritious matter from the floor of the fourth ventricle, and drawing the upper part of the posterior column aside by widening the posterior longitudinal groove, or, what will answer better, by detaching one of

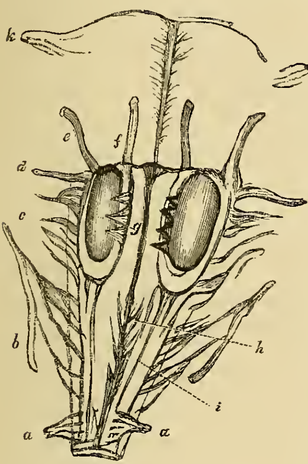

Fig. 4.[1]

[1] Fig 4. *a a* Anterior roots of spinal nerves. *b* Spinal accessory nerve. *c* Par vagum. *d* Glosso-pharyngeal. *e* Portio dura. *f* Abducens oculi. *g* Hypoglossal. *h* Fifth pair. *i* Olivary column. *k* Decussation of pyramidal columns.

[2] Rolando and Mr. Solly (The Human Brain, &c., pp. 148, 149) deny the existence of this olivary tract; but this appears to us a point about which no doubt can be entertained.

the posterior columns at this part, (Fig. 5,) we obtain a posterior view of the decussating fibres of the pyramidal columns already described, as they pass into the posterior part of the middle columns. If we were restricting our examination to this surface alone, we might be led to believe that the decussating fibre brought into view formed a decussation between the two middle columns, and it has been described as such.[1] No doubt, if we describe the middle column as proceeding from below upwards, as is now most frequently done, it may be justly said that this is a decussation of part of the fibres of the middle column; but then it ought to be added, that these decussating fibres do not pass into the middle column of the opposite side, but into the pyramidal column of the opposite side. After the most careful search, we have not been able to find any other decussation besides that we have mentioned. With these remarks upon the anatomy of the *medulla oblongata*,

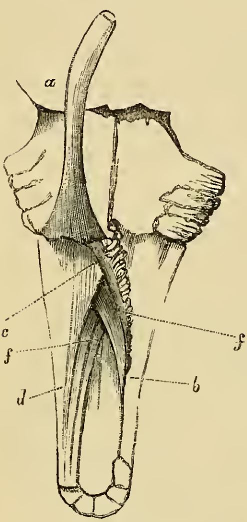

Fig. 5.[2]

we now proceed to point out the attachments of the various nerves to the different columns. We have stated above, that the olivary columns converge towards each other at the lower part of the pyramidal bodies, in consequence of the greater part of the pyramidal columns passing backwards to join the middle lateral columns. If we trace these olivary columns downwards, we observe that they

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[1] The Nervous System of the Human Body. By Sir Charles Bell, p. 218. 1836.

[2] Fig. 5. *Posterior view of decussation.* *a* Posterior column of one side detached and thrown upwards. *b* Posterior column of opposite side in its natural position. *c* Mass of cineritious matter. *f f* Decussating fibres.

afford attachments to the anterior roots of the first and second cervical nerves, (Fig. 4,) and that they continue their course onwards in the line of the other anterior roots of the spinal nerves. We can then have little difficulty in arriving at the conclusion, that the olivary is a motor column. On tracing this column upwards, (Fig. 6,) we

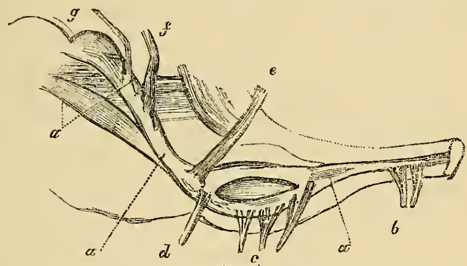

Fig. 6.[1]

find that where it embraces the olivary body, the *portio dura* is attached to its outer margin, and the hypoglossal and abducens nerves are partly attached to its inner margin, and partly to the outer margin of the pyramidal column. On tracing the two roots of the fifth pair, the smaller or motor root can be followed to that portion of the olivary column which proceeds to the optic lobes—sometimes running down the outer or external edge of the pons to reach the portion of the olivary column already mentioned, as it immerges from the external margin of the pons, at other times its course is obscured by a greater or less number of the transverse fibres of the pons crossing it. The trochleator nerve is attached to the internal margin of the same band of fibres when it has ascended the *processus a cerebello ad testes*, and is about to enter the optic lobes. The larger or sensiferous root of the fifth, on the other hand, proceeds downwards, and passes beneath a thick

[1] Fig. 6. *a a a* Olivary column. *b* Anterior root of first spinal nerve. *c* Hypoglossal nerve. *d* Abducens nerve. *e* Portio dura. *f* Smaller root or motor part of the fifth pair. *g* Trochleator nerve.

mass of the transverse fibres of the pons to reach the middle or restiform column, to which it is undoubtedly attached; and to this same column the whole of the fibres of the glosso-pharyngeal, and nearly all the fibres of the *par vagum* are attached. In those cases where the arciform filaments are well marked, (Fig. 3,) we have observed a few of the filaments of the *par vagum* attached to them as they cross the middle column, and it is probable that the few motor filaments contained in this nerve are derived from this source. The filaments of the *par vagum* are certainly not connected with the olivary bodies, and these cannot be the ganglia of these nerves, as Mr. Solly has supposed.[1]

With regard to the origin of the spinal accessory nerves, I find it a more difficult matter to speak with precision; but there can be no doubt that the decussating fibres between the pyramidal and middle columns run down in the course of the spinal accessory, and we can easily imagine how the filaments of this nerve could reach these decussating fibres.

It would be an interesting point to ascertain the functions of these decussating fibres, but this, from their position in the cranium, would be a matter of great difficulty. Their connexion with the root of the spinal nerves is not

[1] Opus cit., pp. 146, 147, 148. Mr. Solly adduces in favour of this view the "amazingly developed" *corpora olivaria* of the porpoise, (p. 101.) In this, however, we very strongly suspect he is mistaken, for while dissecting the brain of a porpoise, we carefully looked for these large *corpora olivaria*, and could not observe them. We have also heard urged in favour of this view, the statement made by Tiedemann in his work on the fœtal brain, that the *corpora olivaria* are imperfectly developed at the sixth month—a period at which there is no use for these nerves. Whether this argument ought to be considered legitimate or not, we need not inquire; but we have two preparations in our possession procured from fœtuses, where we had undoubted evidence that neither of them had passed the sixth month of utero-gestation, and in both the *corpora olivaria* are very prominent, and relatively as well developed as in the adult.

sufficiently obvious to enable us to draw any decisive conclusions regarding their function; and though we have described them as proceeding from the pyramidal to the middle columns, yet we may perhaps with equal justice describe them as proceeding from the middle to the pyramidal columns. In two kittens deprived of voluntary motion and sensation by a dose of prussic acid, I observed extensive muscular movements on irritating the upper part of the pyramidal bodies with the point of a needle; but whether these belonged to the class of excito-motory movements of Dr. Marshall Hall, or whether they resulted from the impression being conveyed outwards directly to the muscles moved, I could not determine. If it should be afterwards ascertained that this is an excito-motory column, it might open up a new field of investigation.

Though the decussation at the lower part of the pyramidal bodies in even all the late anatomical works of this country is still considered as a decussation merely between the two anterior columns, yet it appears that it was correctly described by Fr. Rosenthal in 1815.[1] He states that the pyramidal bodies ascend as a small column at the sides of the anterior fissure of the *medulla spinalis;* that they become thicker and broader at the extremity of the olivary bodies; and that "the sudden increase of its fibres is caused by the accession of small bundles which arise deeply from the large posterior fasciculus of the great column. It is these small bundles which decussating become interwoven with each other, so that several of those of the left side actually pass into the fibres of the right pyramidal body, and *vice versa.*" Cruveilhier[2] states, that the ante-

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[1] Ein Beitrag zur Encephalatomie, Weimar, 1815.—When exhibiting the preparations from which the above description was drawn up, to Dr. Sharpey while on a visit to Edinburgh last autumn, he informed me of the nature of Rosenthal's work, and kindly sent me his copy for perusal.

[2] Anatomie Descriptive, tom. iv., p. 594. Paris, 1835.

rior pyramidal bodies are certainly not formed by a continuation of the anterior columns of the spinal cord, but by fibres from the lateral columns. Arnold's[1] description of the decussating fibres does not differ essentially from that of Rosenthal.

[Mr. Solly, in the second edition of his excellent work on "The Human Brain, its Structure, Physiology, and Diseases," &c., published in 1847, has examined the various anatomical points discussed in this paper, and I am glad to find that he agrees with me in all those parts of the anatomy of the medulla oblongata, which it was chiefly written to illustrate. At page 216, he says, "Dr. John Reid, in an interesting paper, entituled, 'On some points in the Anatomy of the Medulla Oblongata,' was, I believe, the first to point out that the 'olivary is a motor column.' I have carefully investigated this point since my first edition: I fully agree with him; also in the important fact that the motor root of the fifth pair arises from it, which I made out, without being aware that he had previously discovered the same. The origin of the motor, or non-ganglionic root of the fifth pair of nerves, has long puzzled anatomists, and we find a different description in most anatomical works." Mr. Solly, at page 240, says, "Dr. Reid describes, and I think justly, the pyramidal columns as passing into the middle columns after their decussation; he describes the arciform fibres as going to join the posterior or cerebellar column, as I first described them, but he is in error in attributing to Santorini, Gall, and Spurzheim, Rosenthal, and Rolando, a knowledge of the fact that they

[1] Bemerkungen über den Bau des Hirns und Rückenmarks, &c., Zurich, 1838. S. 30. In his Tabulæ Anatomicæ Fasciculus, i., Tab. ix. Fig. 1, he represents the pyramidal bodies as being formed entirely by fibres from the lateral column.

form a communication between the anterior portion of the cord and cerebellum, which I consider an important anatomical discovery alone due to me." I think it due to Mr. Solly to transcribe the above paragraph. If I have deprived him of any part of the credit to which he is justly entitled, I did it unwittingly. At pages 222 and 243, Mr. Solly agrees with me in thinking that Sir Charles Bell, "in all probability, mistook the posterior surface of the pyramidal decussation for a distinct decussation." At page 239, Mr. Solly gives, and fully adopts, the description I have given of the attachments of the motor encephalic and motor spinal nerves to the olivary column.]

No. VIII.

ON THE ANATOMICAL RELATIONS OF THE BLOOD-VESSELS OF THE MOTHER TO THOSE OF THE FŒTUS IN THE HUMAN SPECIES.

(FROM THE EDINBURGH MEDICAL AND SURGICAL JOURNAL, NO. 146, JAN. 1841.)

THE particular manner in which the blood-vessels of the mother and fœtus are arranged on the inner surface of the uterus and in the interior of the placenta, by which their contained fluids act and react upon each other, and the blood of the fœtus is rendered fit for the process of nutrition, has been the subject of much discussion, and of much investigation. That the blood-vessels of the uterus and placenta communicate with each other, and that a mutual interchange of blood can readily take place between them, appears to have been the prevailing supposition among the earliest cultivators of physiological science. This opinion was afterwards strengthened by the results which Cowper,[1] Vieussens,[2] Noortwyk,[3] and others, supposed they had obtained from injections of impregnated uteri; and also by the details of cases in which it was believed that the blood-vessels of the fœtus had been drained by profuse or fatal

[1] The anatomy of the Human Body. The fifty-fourth Table. 1698.

[2] Dissertatio de Structura et Usu Uteri Placentæ Muliebris, in Genevensi Verheyenii editione. Vieussens' observations were made on the bitch.

[3] Uteri Humani Gravidi Anatome et Historia, p. 10. 1743.

hæmorrhage from the mother.[1] Haller has adduced several arguments to show that there must be some direct vascular connexion between the mother and fœtus;[2] and Senac maintained that the uterine vessels of the mother, and the placental arteries of the fœtus, poured their blood into a cellular tissue in the placenta, and that the fœtal placental veins took their origin from the interior of these cells.[3] Flourens is the only author of any reputation in modern times who has alleged that there is any direct communication between the blood-vessels of the mother and the fœtus; but there cannot be a doubt that this opinion is erroneous, and ought now to be totally abandoned.[4]

Satisfactory evidence was adduced by Monro *primus*,[5] the two Hunters,[6] Monro *secundus*,[7] and his brother, Dr. D. Monro,[8] and by Wrisberg,[9] that there is no vascular continuity between the blood-vessels of the mother and the placental vessels of the fœtus, and this has since been most fully confirmed by the testimony of numerous accurate and careful observers.

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[1] *Vide* a case by Mery, Mémoires de l'Académie Royale des Sciences. 1708.

[2] Elementa Physiologiæ, tom. viii., lib. xxix., sect. iii. We are informed by Dr. D. Monro (Edinburgh Physical and Literary Essays, vol. i., p. 456. 1771,) that Haller, in a private conversation, stated to him, that subsequently to the expression of this opinion, he had examined three impregnated uteri, and found nothing like a direct communication between the uterine and fœtal placental vessels, "so that he now believed that there was no such anastomosis as was alleged."

[3] Traité de la Structure du Cœur, tom. ii., p. 68. 1749.

[4] Cours sur la Génération, p. 130. Paris, 1836. Flourens states that this communication exists in those animals only which have a *single* placenta, as in the human species, the carnivora, and the rodentia.

[5] Edinburgh Medical Essays, vol. ii., p. 102. Third edition. 1747.

[6] John Hunter "On the Animal Economy," 1794; and William Hunter, "The Anatomical Description of the Human Gravid Uterus and its contents." 1794.

[7] Edinburgh Physical Essays, vol. i., p. 481.

[8] Idem liber, p. 456.

[9] Commentationum Medici, &c., pp. 46 and 312. 1800. See more especially his notes to the Primæ Lineæ Physiologiæ of Haller, Caput xxxi.

A most important advance was made in our knowledge of the anatomical relation of the blood-vessels of the mother and fœtus by the labours of the Hunters. They satisfied themselves that the umbilical arteries terminate in the umbilical veins, and not in the vessels of the uterus, and that the blood in the umbilical arteries "passes from the arteries into the veins as in other parts of the body, and so back again into the child." They further observed, that numerous small curling arteries, the largest being about the size of a crow-quill, passed from the inner surface of the uterus, that they penetrated the decidua, and opened into the interstices between the fœtal blood-vessels of the placenta.[1] Prolongations from the uterine sinuses were also traced through the decidua, and were observed to terminate in the placenta in the same manner as the curling arteries, so that "in the umbilical portion of the placenta the arteries terminate in veins by a continuity of canal, whereas in the uterine portion there are intermediate cells, in which the arteries terminate, and from which the veins begin." It was therefore concluded that the blood of the mother was poured by the curling arteries into a kind of cellular tissue, filling up the intervals between the ramifications of the fœtal placental vessels, from which it returns to the uterine sinuses of the mother through their placental prolongations, after having acted upon the blood of the fœtus through the thin walls of the umbilical placental vessels.

The existence of these utero-placental vessels has, however, within the last few years, been repeatedly called in question, both in this country and on the Continent, and it has been asserted that the blood of the mother cannot be carried into the interior of the placenta, that the placenta

[1] These curling arteries have been figured by Albinus (Uteri Gravidi, Tab. vii.;) by Dr. D. Monro and Monro *secundus* (Opera cit.;) by William Hunter (The Anatomy of the Gravid Uterus Exhibited in Figures, plates 28, 29, and 30. 1774;) and by John Hunter (opus cit.)

is entirely a fœtal organ, and that the blood-vessels of the uterus and placenta cannot intermingle. The supporters of this view of the nature of the connexion of the uterus and placenta have been Lauth,[1] Dr. R. Lee,[2] Velpeau,[3] Dr. Radford,[4] Seiler,[5] Ramsbotham,[6] Mr. Millard,[7] and Mr. Noble.[8] From the extent and respectability of this adverse testimony, serious doubts of the correctness of the description given by the Hunters began to be entertained by many who had hitherto placed implicit confidence in it. Evident signs of a rapid return to the Hunterian views have, however, now manifested themselves, and this has been effected not only by the confidence due to the known accuracy of the Hunters, but also by the late observations of Weber,[9] Dr. Hugh Ley,[10] Eschricht,[11] Mr. Owen,[12] and others; and by the report made by Mr. Mayo and Mr. Stanley on the preparations of impregnated uteri contained in John Hunter's Museum.[13]

Though the researches of Weber and Eschricht have

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[1] Répertoire Général D'Anatomie et de Physiologie, tom. i., p. 75. 1826. Lauth believed that the union of the uterus and placenta is effected by means of vessels, but these are lymphatics and not blood-vessels.

[2] Philosophical Transactions for 1832, p. 57.

[3] Embryologie ou Ovologie Humaine, p. 63. Paris, 1833, "J'ai cherché mais en vain ces vaisseaux utero-placentaires," p. 68.

[4] On the Structure of the Human Placenta. Manchester, 1832.

[5] Die Gebärmutter und das Ei des Menschen, S. 31. Dresden, 1832.

[6] Medical Gazette, vol. xiii., p. 613. 1834.

[7] *Ibid.* vol. xiv., p. 654. 1834.

[8] *Ibid.* p. 810.

[9] Hildebrandt's Handbuch der Anatomie des Menschen. Vierter Band. S. 495. 1832.

[10] Lancet, 15th June, 1833.

[11] De Organis quæ Respirationi Fœtus Mammalium inserviunt; Prolusio Academica. Hafniae, 1837. *Vide* Wagner's Lehrbuch der Physiologie, Erste Abtheilung, S. 124. 1839.

[12] Palmer's edition of John Hunter's Works, vol. iv., p. 67. Mr. Owen in his first investigations into this subject adopted the conclusions of Dr. Lee, but more lately he has seen good grounds to return to the Hunterian view.

[13] Lancet, 22d June, 1833.

led them to adopt the description of the Hunters, as far as relates to the existence of the utero-placental vessels, yet their account, as we shall afterwards see, of the structure of the interior of the placenta, differs considerably, not only from the Hunters, but also from each other.

Having made these preliminary remarks, we shall now proceed to give the details of an examination of an impregnated uterus which we had lately an opportunity of making, as they appear to us to throw additional light on these intricate questions. The uterus was obtained from the body of a woman who died suddenly about the middle of March 1839, from apoplexy occasioned by inflammatory softening of the brain, while near the seventh month of pregnancy. The uterus was first opened, the fœtus was then removed, and a portion of the uterus with the whole of the adhering placenta was carefully secured. The principal facts, contained in the description which I am about to give, were not made out until August 1840. On separating the adhering surfaces of the uterus slowly and cautiously under water, I satisfied myself, but not without considerable difficulty, of the existence of the utero-placental vessels described by the Hunters. After a portion of the placenta had been detached in this manner, my attention was attracted towards a number of rounded bands passing between the uterine surface of the placenta and the inner surface of the uterus. These bands were generally observed to become elongated, thinner, and of a cellular appearance when put upon the stretch, and were easily torn across; while at other times, though much more rarely, they could be drawn out in the form of tufts from the mouths of the uterine sinuses. On slitting up some of the uterine sinuses with the scissors, these tufts could be seen ramifying in their interior, and were more or less elongated; many of them appearing only to dip into the open mouths of the sinuses, while others proceeded from a quarter of an inch to an inch from the open

mouths of the sinuses by which they had entered, and in some cases they extended themselves into one of the neighbouring sinuses. (Fig. 1.) The next point was to

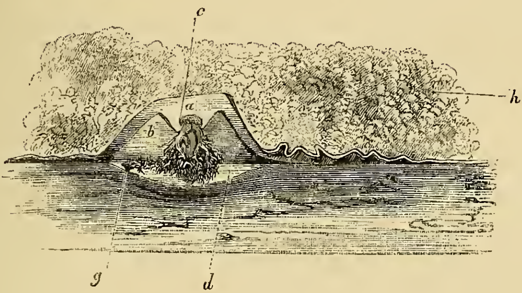

Fig. 1. [1]

endeavour to ascertain the nature of these tufts by injection and microscopic examination. A size injection was thrown into the umbilical vein, and though it ran imperfectly from the injury done to the detached portions of the placenta while tracing the course of the utero-placental vessels, yet several of the branches of the tufts contained in the uterine sinuses were filled with injection, and their continuity with the umbilical placental vessels was clearly ascertained. On placing portions of these tufts under the microscope, along with portions of the umbilical vessels taken from different parts of the interior of the placenta, their identity was at once apparent. Having thus determined that these tufts observed in the uterine sinuses of the mother were prolongations of the fœtal placental vessels, I then proceeded to examine their anatomical relations to these sinuses. These tufts were found to protrude into the open mouths of certain of the uterine

[1] Fig. 1. *Actual representation of a uterine sinus containing tufts of fœtal placental vessels.* *a Membrana decidua vera* turned up from the inner surface of the uterus. *i* Walls of uterus. *d* Tufts of placental vessels in uterine sinus. *b* Walls of uterine sinus which have been cut through and laid back. *e* Opening in the decidua, through which the placental vessels passed into the sinus. *g* Opening through which other tufts passed into the same sinus. *h* Placenta.

sinuses only, and it need scarcely be added that they were observed only in those sinuses placed next the inner surface of the uterus, and not in any of the deeper sinuses. These tufts were surrounded externally by a soft tube similar to the soft wall of the utero-placental vessels, which passed between the margin of the open mouths of the uterine sinuses, and the edges of the orifices in the decidua through which the tufts protruded themselves into the sinuses. The size of these tufts varied considerably. Some of them appeared to fill up completely the open mouths of the sinuses by which they entered; while others filled them only partially.[1] On examining these tufts as they lay in the sinuses, it was evident that, though they were so far loose and could be floated about, yet they were bound down firmly at various points by reflections of the inner coat of the venous system of the mother upon their outer surface. This reflection of the inner coat of the uterine sinuses upon the tufts was sometimes observed at the point where these entered the open months of the sinuses, at other times it was at or near their apices, and was in general so strong that the tufts were torn across in attempting to detach them by pulling. In this uterus we thus ascertained that while some of the utero-placental veins contained no prolongations of the fœtal placental vessels, in others these passed along their interior and projected into the uterine sinuses. On tracing those utero-placental veins, which contained no fœtal vessels, as far as the placental surface of the decidua, the inner coat of the venous system was seen to be prolonged upon some of the tufts of fœtal-placental vessels in their immediate neighbourhood. On tracing one of the larger of the curling arteries through the decidua, it was also observed, that when it reached the placental surface of that membrane, the inner coat of the arterial system

[1] The mouths of the sinuses form rounded openings, which are much smaller than the interior of the sinuses into which they open.

of the mother was prolonged upon some of the tufts of the fœtal placental vessels, which projected into their orifices. Those numerous branches of the fœtal placental vessels, which reach the placental surface of the decidua, and do not pass into the uterine sinuses, nor into the orifices of the utero-placental vessels, are attached by their apices to the placental surface of that membrane.

On placing some of the filaments, composing these tufts of fœtal placental vessels found in some of the uterine sinuses of the mother, under the microscope, they were observed to divide and subdivide into branches more or less elongated, all of which terminated in blunt extremities. Fig. 2 is an exact representation of a branch of one of these tufts as it was observed through the microscope, and was taken by my friend, Mr. John Goodsir, of whose very valuable testimony I have availed myself in almost all the observations I have made on this subject. Few, however, of these branches afforded so favourable an opportunity of observing their mode of termination, for they are almost always more elongated and tortuous than they are here represented, and, from being interlaced, it is generally impossible to unravel them without tearing or otherwise altering their form. A similar examination of the vessels composing the mass of the placenta showed that these presented the same anatomical characters, though, from the intricate manner in which their branches are there interlaced, without, however, having any actual attachment to each other, as they cross and recross each other, there would have been much difficulty

Fig. 2.[1]

[1] Fig. 2. Actual representation of one of the branches of a tuft removed from one of the sinuses, as seen through the simple microscope.

in ascertaining this, had those found in the uterine sinuses not been first examined, since they are there in general much shorter and less interwoven than in the placenta itself.[1] It would be exceedingly difficult to give any exact representation or description of the arrangement of the fœtal placental vessels, for the subdivisions of the larger branches are so numerous, vary so much in length and in tortuosity, and the only point which we are anxious to establish at present is, that, notwithstanding those varieties in the subdivision of their branches, they are all found to terminate in blunt extremities. There is no cellular nor any other tissue filling up the intervals left between the branches of the fœtal placental vessels; and the difficulty experienced in unravelling them does not arise from the presence of any connecting medium, but from the crossing and recrossing of the branches of different tufts, and those of the same tuft with each other. The outer surface of the placental vessels has a smooth appearance, and they are, *we may suppose*, everywhere enveloped in the inner coat of the vascular system of the mother, which, as we have seen above, is reflected upon them.

As the blood-vessels forming the placenta, and also those prolonged from it into the uterine sinuses, divide and subdivide into numerous branches, which do not anastomose with each other, but all terminate in blunt extremities, we might on theoretical grounds, as well as from the descriptions given by others, decide that in each of these branches an artery and a vein are bound up together. This conclusion was fully confirmed by the microscopic examination of some of the injected branches of the tufts of the placenta

[1] The placental tufts have an evident resemblance to the representations of the branches of the shaggy corion, given by Raspail, (Organic Chemistry, Plate xiii. Fig. 9): by Ritgen (Beiträge zur Aufhellung der Verbindung der menschlichen Frucht mit dem Fruchthälter, Tab. iii. Fig. 5 and 6, 1835,) and by Seiler, (Die Gebärmutter und das Ei des Menschen, Tab. xi. Dresden, 1832.)

which passed into the uterine sinuses, and also of three other placentæ, in which the arteries and veins were filled with differently coloured size injection. I was satisfied that each of the smaller branches of the placental arteries is bound up with another branch of one of the placental veins, which closely accompanies it, forming what appears a single vessel when viewed uninjected through the microscope. Each branch of the umbilical artery is thus closely bound up with a branch of the umbilical veins, and both of them divide and subdivide exactly in the same manner, and terminate in what appear to be blunt extremities, but which actually form the termination of the arteries, and the commencement of the veins. The interior of the placenta

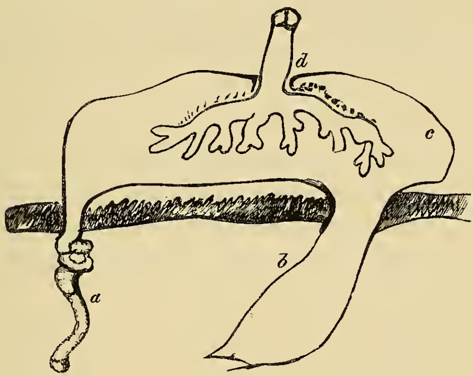

Fig. 3.[1]

is thus composed of numerous trunks and branches, (each including an artery and an accompanying vein,) every one of which, we believe, is closely ensheathed in prolongations of the inner coat of the vascular system of the mother, *or at least in a membrane continuous with it.* If we adopt this view of the structure of the placenta, the inner coat of the vascular system of the mother is prolonged over each in-

[1] Fig. 3. *Diagram to illustrate the views here given of the structure of the placenta.* *a* Curling artery. *b* Uterine vein. *c* Placenta. *d* Placental tufts, with inner coat of vascular system of the mother enveloping them.

dividual tuft, so that when the blood of the mother flows into the placenta through the curling arteries of the uterus, it passes into a large sac formed by the inner coat of the vascular system of the mother, which is intersected in many thousands of different directions, by the placental tufts projecting into it like fringes, and pushing its thin wall before them in the form of sheaths, which closely envelop both the trunk and each individual branch composing these tufts. From this sac the maternal blood is returned by the utero-placental veins without having been extravasated, or without having left her own system of vessels. Into this sac in the placenta containing the blood of the mother, the tufts of the placenta hang like the branchial vessels of certain aquatic animals, to which they have a marked analogy. This sac is protected and strengthened on the fœtal surface of the placenta by the chorion; on the uterine surface by the *decidua vera*, and on the edges or margin by the *decidua reflexa*.[1] The blood of the mother contained in this placental sac, and the blood of the fœtus contained in the umbilical vessels, can readily act and react upon each other through the spongy and cellular walls of the placental vessels, and the thin sac ensheathing them, in the same manner as the blood in the branchial vessels of aquatic animals is acted upon by the water in which they float. According to this view of the structure of the placenta, the fœtal and maternal portions of the placenta are everywhere intimately intermixed, and we find tufts of minute placental vessels with their blunt

[1] We have no hesitation in following William Hunter in regarding the membrane on the uterine surface of the placenta as the *decidua vera*, notwithstanding all the assertions by Velpeau to the contrary. We have in our possession a preparation of a uterus at the fifth week of utero-gestation, where a thick layer of the *decidua* evidently intervenes between the shaggy chorion surrounding the ovum, and the inner surface of the uterus; and it was also observed that some of the tufts of the shaggy chorion had insinuated themselves into openings in the decidua.

terminations, lying immediately under the chorion covering its fœtal surface, as well as towards its uterine surface. When the fissures dividing the placenta into lobes are so deep as to intersect the whole thickness of the placenta, we may have two or more of these sacs instead of one. The discovery of the prolongations of the fœtal placental vessels into some of the uterine sinuses is principally valuable as it presents us with a kind of miniature representation of the whole structure of the placenta, and enables us

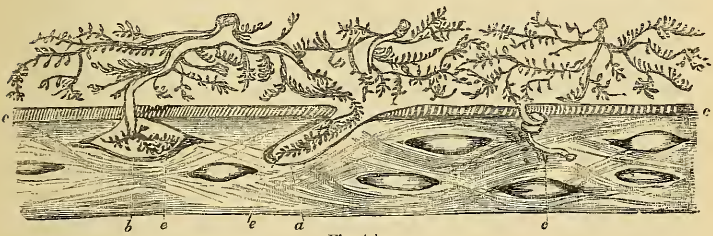

Fig. 4.[1]

to comprehend it readily, for we have there the fœtal blood-vessels, resembling branchial vessels, ensheathed in the inner coat of the vascular system of the mother, and bathed in the maternal blood. The placenta is therefore not analogous in its structure to the lungs, but to the branchial apparatus of certain aquatic animals.

It may at first appear remarkable, that, if tufts of the placental vessels are prolonged into the uterine sinuses, these are not observed projecting from the uterine surface of the placenta when it has been expelled from the uterus in an accouchement. This, however, is explained by the fact, of their being so strongly bound down by the reflection of the inner coat of the uterine sinuses, that they are torn across. As hæmorrhage does not occur from vessels of this size when torn across, we can also explain how fine

[1] Fig. 4. *Diagrammatic sketch of a transverse section of the uterus and placenta.* *a* and *b* Uterine sinuses, with tufts of fœtal placental vessels prolonged into them. *c* A curling artery passing through the *decidua vera.* *c Decidua vera.* *e* Tufts of placental vessels.

injections thrown into the placental vessels do not flow from their broken extremities, and how, in certain cases where the placenta and child have been expelled together, and where the circulation through the placenta was allowed to proceed for some time, the blood did not escape at its uterine surface. In a uterus obtained from a woman who died twenty-four hours after delivery, and which I had an opportunity of examining through the kindness of Professor Simpson, I observed that, while part of the mouths of the uterine sinuses were blocked up with coagula, a considerable number of them were empty. In those which were empty I could detect no tufts, while in those filled up with the coagula I distinctly observed several tufts of placental vessels enveloped in the coagula, when they were placed under the microscope and broken up.

In detaching the placenta from the uterus, the strongest points of adhesion were found at the place where the tufts were bound down by the reflection of the inner coat of the uterine sinuses, and it is possible that, in some cases, unnatural adhesions of the placenta may be caused by these tufts undergoing the cartilaginous degeneration, and not only be rendered stronger themselves, but also cause a thickening of the membrane reflected upon them.[1]

I regret that I have not had an opportunity of verifying these observations upon other impregnated uteri, for in the various quarters in which I have made inquiries after such preparations, I have hitherto been unsuccessful in procuring one.

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[1] In a placenta containing some cartilaginous masses, into which I had thrown a fine injection, I observed, on unravelling one of these after it had been macerated for some days in water, that the injection had only entered the larger trunks of the vessels, and that all the smaller ones were impermeable, and had the colour and consistence of the cartilaginous mass itself, which was in fact entirely composed of them.

[2] It is quite possible that these tufts do not generally project so far into the interior of the uterine sinuses as in the uterus we have examined

It appears to have been a common theoretical opinion, during the first half of the 18th century, that the fœtal placental vessels passed through apertures in the membrane placed on the uterine surface of the placenta (then generally termed chorion,) and fixed themselves in orifices in the inner wall of the uterus. Brendel[1] describes the radicles of the chorion as implanting themselves in the inner surface of the uterus, in the same manner as a plant fixes itself in the soil, and in this manner forms blood-vessels which take up a nutritious lymph, which they receive from the maternal vessels of the uterus. Trew also describes the filaments of the placenta as proceeding into the substance of the uterus. Rouhault[3] describes the membrane on the uterine surface of the placenta as "percée dans toute son étendue pour laisser passer les racines des vaisseaux ombilicaux, qui vont ou qui viennent de toutes les parties de la matrice par un nombre innombrable de petits trous." That this description does not refer to the fœtal placental vessels passing into the uterine sinuses which we have above described, but to certain imaginary vessels, is still further evinced by the statements contained in another paper by the same author, where the membrane on the uterine surface of the placenta is said to be pierced by a *million* of small holes, each of which is filled up by as many small capillary blood-vessels.[4] Vater,[5] proceeding on the supposition which we have seen to be common at the time he lived, viz. that the ovum, after exhausting the nutriment which it found in the uterus, sends the filaments of the

[1] De Nutritione Fœtus in Utero Materno, published in 1704, reprinted in Haller's Disput. Anatom. Select., tom. v., p. 488.

[2] De Chylosi Fœtus in Utero, published in 1715, reprinted in Haller's Disput. Anat., tom. v., p. 445.

[3] Mémoires de l'Académie Royale des Sciences, 1716, p. 343.

[4] Idem liber, 1714, p. 182.

[5] De Utero Gravido, published in 1725, Haller's Disput. Anat., tom. v., p. 260.

chorion into the substance of the uterus as a plant shoots its roots into the soil, and being also aware of the existence of the open mouths of the uterine sinuses, states, that the vascular extremities of the chorion insinuate themselves into the *open mouths of the uterine sinuses*, and draw nourishment from them. We find no evidence which would lead us to believe that this description given by Vater was not a mere expression of a theoretical opinion. Noortwyk[1] describes numerous vessels emerging from the uterine surface of the placenta, inserting themselves into the uterine pores, and forming a manifest communication between the blood-vessels of the mother and fœtus. He afterwards states that these vessels are not continuations of the capillary vessels of the placenta, but vessels "omnino sui generis."[2] Monro *primus*,[3] after maintaining that the placenta is covered on the uterine surface by a fine membranous continuation of the chorion, states, that "the extremities of the umbilical vessels pierce this membrane, and show their very small orifices on the side next to the uterus, and therefore it is compared to the villous coat of the intestines." J. G. D. Michaelis[4] describes the fœtal placental vessels penetrating the decidua, and passing into the walls of the uterus. He further adds, that some of them "ad vasorum uterinorum *orificia lateralia quasi agglutinatæ* firmiter sese applicant." We have thus seen that a theoretical notion prevailed for a considerable time, that the fœtal vessels passed through minute and numerous orifices in the decidua; that it was then also generally believed that they entered into small openings in the inner surface of the uterus; and that one author made the for-

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1 Uteri Humani Gravidi Anatome, &c., p. 10. 1743.

2 Idem liber, p. 14.

3 Edinburgh Medical Essays, vol. ii., p. 102. 1747.

4 Dissertatio Inauguralis, Med. De Placenta Humana. The date of this dissertation was obliterated in the only copy which I have seen, but the author was born in 1756.

tunate guess that they passed into the open mouths of the uterine sinuses.[1]

It will be at once apparent to those who are familiar with the subject, that the view here given of the anatomical relations of the blood-vessels of the mother to those of the fœtus is considerably different from that proposed by the Hunters, and by those who have succeeded them. The Hunters were not aware that the fœtal placental vessels are prolonged into the uterine sinuses of the mother;[2] and they believed that the intervals between the fœtal placental vessels were filled up by a cellular tissue, into which the maternal blood is poured—an opinion which has been adopted by many on their authority, and has lately received the sanction of Mr. Owen.[3] This cellular structure of the placenta is described by William Hunter as being formed by processes of the decidua, prolonged everywhere in the substance of the placenta, "shooting out into innumerable floating processes and rugæ, with the most irregular and most minutely subdivided cavities between them that can be conceived;"[4] and he further adds, that the decidua constitutes the uterine portion of the placenta, and forms a number of cells like the *corpora cavernosa penis*, communicating freely with each other.[5] Eschricht[6] supposes that the utero-placental vessels divide and subdivide in the placenta like the arteries and veins in other parts of the body. The description which approaches

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[1] It is quite possible that this idea may have been repeated by other authors, of whose works I am ignorant.

[2] "It is evident that the umbilical injected vessels do not reach even the outer surface of the placenta, but are only seen through a membrane (decidua) which covers all that surface." William Hunter, opus cit., p. 42

[3] Opus cit., p. 69. [4] Opus cit., p. 43.

[5] This description of the maternal part of the placenta appears to have been adopted by Meckel, (Manuel d'Anatomie Gén. et Descrip., tom. iii., p. 764,) Burns, (Principles of Midwifery, 7th edition, p. 189,) and many others.

[6] Opus cit.

nearest to ours is that given by Weber,[1] but they differ in what I conceive to constitute some of its leading peculiarities. Weber denies that the fœtal placental vessels extend beyond the decidua.[2] He states that the inner coat of the venous system of the mother is prolonged into the placenta, but he describes it as ramifying in the intervals of the placental tufts, in the form of large venous sinuses, upon the walls of which the placental tufts are not only ramified, but also project into their interior, carrying the walls of the sinuses before them. He compares the bronchial tubes of the lungs to the ramifications of the utero-placental vessels in the placenta, and those of the fœtal placental vessels to the pulmonary vessels, with this difference, that the utero-placental vessels do not subdivide into numerous small branches like the bronchial tubes, but form large and wide canals.[3] Wagner in a late work appears to adopt Weber's views, and describes the utero-placental blood-vessels as winding in an expanded net-work around the tufts of the chorion containing the vessels of the embryo, and this net-work, he says, is formed of peculiar but very delicate tubes, of large calibre, especially in the case of the veins.[4]

Note.—The transverse sections of the uterus and placenta in my possession have been examined by many who have paid attention to this subject, among whom I may mention Professors Alison, Allen Thomson, and J. Y. Simpson; and they have all expressed themselves satisfied that the placental tufts were prolonged into the uterine sinuses, and that the inner coat of the veins was prolonged upon them. I have just learned from Professor Sharpey, who was so kind as to examine, at my request, two preparations of impregnated uteri in his possession, that in one of these, which had apparently arrived at the full period of utero-gestation, and was in a condition more favourable for examination, he distinctly observed the placental tufts projecting into the mouths of the uterine sinuses.

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[1] Hildebrandt's Anatomie des Menschen. Vierter Band, S. 495.

[2] "Der Uterustheil ist der an dem Mutterkuchen anliegende Theil der Tunica Decidua Vera, in welchen sich die Flocken des Fötustheils der Placenta nicht erstrecken." S. 495.

[3] Opus cit., p. 500.

[4] Lehrbuch der Physiologie, Erste Abtheilung, S. 123. 1839.

APPENDIX

TO

PAPER ON THE ANATOMICAL RELATIONS OF THE BLOOD-VESSELS OF THE MOTHER TO THOSE OF THE FŒTUS.

(EDINBURGH MEDICAL AND SURGICAL JOURNAL, JANUARY, 1841.)

I HAVE had to-day an opportunity of examining an impregnated uterus, through the kindness of the Curators of the Museum of the Royal College of Surgeons here, and I beg to add the results of that examination as an Appendix to my paper on the Relations of the Blood-vessels of the Mother and Fœtus. It was apparently between the fourth and fifth months of pregnancy, and had been preserved for a long time in spirits. It had been cut open in front to show the interior, and in doing this a section had been made of that part of the uterus to which the placenta adhered. To avoid injuring the preparation, I examined it only along the cut edge. I found that an injection had been thrown into the uterine arteries, which only partially filled them. The veins were not injected. Several of the *curling arteries* were well filled with injection, while others contained only a small quantity. On tracing these from the inner surface of the uterus through the decidua, they were observed to terminate among the tufts of the placenta, without undergoing any change in their calibre during their oblique course through the decidua. The injection filled up the intervals between the tufts of the placenta in the immediate neighbourhood of the point of termination of each. These curling arteries were not continued into the placenta in the form of a tube or dilated vessel, as has been represented; but, from the manner in which the tufts adhered to their edges, we believe that the inner coat was

reflected upon them, as we have already stated. On examining the utero-placental veins, the tufts of the placenta were in many cases observed to project into their open mouths, and to adhere to their inner surface, but in none that were examined did they project so far as the uterine sinuses. It is possible that if the dissection could have been carried farther, that they might have been found to project along the veins to a greater extent at some other points. In the uterus in my own possession, the tufts in numerous cases projected only into the mouths of the utero-placental veins, while in many others they extended themselves as far as the mouths of the uterine sinuses, and even projected to a considerable distance into their interior. In all these different relations of the placental tufts to the utero-placental veins, the inner coat of the vascular system of the mother is, as far as I have observed, reflected upon the outer surface of these tufts. It is very probable that the placental tufts may project into the utero-placental veins to different distances in different cases, and it may be to a greater extent in the advanced periods of utero-gestation, than at an earlier period. I am the more inclined to adopt this opinion, as it has been suggested to me, not only by the result of this examination, but also by a communication from Dr. Sharpey. However this may be, it does not affect the views which I have advanced regarding the structure of the placenta, for it is apparent that these do not necessarily require that the tufts of the placenta should project into the uterine sinuses. The great extent to which these tufts projected into the uterine sinuses in the impregnated uterus in my possession was chiefly of advantage in enabling me to examine, under very favourable circumstances, the mode in which the placental vessels terminate, the reflection of the inner coat of the vascular system upon the outer surface of these vessels; and in furnishing a miniature representation of the structure of the interior of the placenta.

[I HAVE withdrawn a criticism upon Weber's representation of the fœtal placental vessels given in Wagner's Icones Physiologicæ, contained in a footnote at page 6 of the above paper, as originally printed in the Edinburgh Med. and Surg. Journal. As I have elsewhere explained,[1] that criticism was founded upon a misconception on my part. I had the pleasure of seeing Professor Weber at Leipsic in the summer of 1842, and had an opportunity of examining portions of placentæ injected by him, and I entertain no doubt that his representations of the injected fœtal placental vessels are perfectly correct. Mr. John Dalrymple[2] and Professor Goodsir[3] have testified to the accuracy of the description of the injected blood-vessels of the placenta as given by Weber. Dr. F. Renaud[4] has pointed out that the arrangement of the fœtal placental vessels presents a different aspect in the three conditions in which they are usually examined—viz., when injected, when neither injected nor immersed in any fluid, and when after they have been immersed in water; and in this way he endeavours to reconcile some discrepancies in the descriptions of the arrangement of these vessels given by different observers. Professor Goodsir[5] has given an elaborate description of the placenta, in which he has pointed out some structures previously overlooked. He concurs in the account of the maternal portion of the placenta which we have given in the above paper, and describes the external membrane of the villi as a fine transparent membrane, continuous with the inner membrane of the vascular system of the mother. Immediately under this membrane there is a layer of cells, previously described by Mr. Dalrymple as

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[1] Edinburgh Med. and Surg. Journal for January 1843; and London Medical Gazette a short time previous to this.

[2] London Medico-Chirurgical Transactions, vol. xxv., p. 21. 1842.

[3] Anatomical and Pathological Observations, p. 50. Edinburgh, 1845.

[4] Edinburgh Monthly Journal of Medical Science for 1843, p. 181.

[5] Opus cit.

"nucleated cells resembling an irregular epithelium."[1] This system of cells, according to Mr. Goodsir, belongs to the decidua, and their function is to separate from the blood of the mother the matter destined for the blood of the fœtus. He describes another membrane placed immediately within these external cells of the placental villi, which he terms the internal membrane of the villi. This membrane, he believes, belongs to and bounds the fœtal portion of the placenta. Inclosed within this internal membrane, and lying on the external surface of the fœtal capillary vessels of the placenta, there is another system of cells belonging to the fœtal portion of the placenta, and constituting the internal cells of the placental villi, and their function is to absorb, through the internal membrane, the matters secreted from the maternal blood by the agency of the external cells of the villi.]

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[1] Opus cit., p. 24.

No. IX.

INJECTIONS OF THE VESSELS OF THE FŒTUS, TO SHOW SOME OF THE PECULIARITIES OF ITS CIRCULATION.

(FROM EDINBURGH MEDICAL AND SURGICAL JOURNAL, JAN. AND APRIL 1835.)

THREE fœtuses were injected in the following manner. A red-coloured injection was thrown up the *vena cava* inferior, and a yellow-coloured injection down the *vena cava* superior at the same time, and as much as possible in equal quantity and with equal force, to endeavour to imitate the currents which flow along these veins during the life of the fœtus. As it is impossible that one person can manage both syringes, it was necessary to intrust them to different persons, and this must render the success of the experiment more precarious. It was principally wished by these injections to try to ascertain to what extent the Eustachian valve prevents the intermixture of the two currents entering the right auricle by the two *cavæ;* and, provided that the Eustachian valve really has the effect of keeping the two currents to a certain extent separate, and of directing the greater part of the ascending current into the left auricle through the *foramen ovale*, whether these two currents still continue separate in their subsequent course—that passing along the aorta going entirely to the large vessels of the head and superior extremities, and that through the *ductus arteriosus* filling the descending aorta.

The first trial was made on a fœtus of between five and six months. By some mistake the injection was not thrown down the *cava superior*, and that thrown along the *cava inferior* was in small quantity. On examining the heart, we found that the red injection had passed along the *cava inferior;* that some of it had passed into the right auricle, but the greater part had been directed through the *foramen ovale* into the left auricle by the Eustachian valve, so as to fill the whole of the left side of the heart, while not a single drop of injection had passed into the right ventricle.

This experiment, then, was not entirely useless, for it showed, in this case at least, that a fluid ascending through the inferior *cava* passed more readily into the left side of the heart than into the right. That part of the injection should have passed into the right auricle was to have been expected; for though the Eustachian valve when perfect nearly insulates the *cava inferior* from the cavity of the right auricle, yet it cannot entirely exclude the passage of some of the fluid into the right auricle, when that cavity is not occupied by the column of blood which descends through the superior *cava*.

The second trial was upon a fœtus at the full period. Upon examination we found, that though the two currents chiefly passed in the course which we shall fully describe in the third experiment, yet some intermixture had taken place. This was not, however, to any great extent. This intermixture might depend upon two causes: *First*, the Eustachian valve is supposed to be less perfect at the full time than at an earlier period; and, *secondly*, the injection was not so well-managed as it ought to have been, from the difficulties experienced in two persons commencing and stopping exactly at the same time, and using nearly equal forces.

The third trial was upon a fœtus of about seven months, judging from its size and the position of the testicles. Care

was taken, by a previous course of training, to throw in the two currents as equally as possible. On tracing the red injection upwards, we found that it had passed through the *foramen ovale* and filled the left side of the heart without any intermixture with the yellow, except very slightly at the posterior part of the right auricle. Not a drop of the yellow appeared to have accompanied the red into the left side of the heart. From the left side of the heart the red injection had ascended the aorta, and filled all the large vessels going to the head and upper extremities. The injection in all these vessels had not the slightest tinge of yellow.

On tracing the yellow downwards, we found it filling the right auricle, free from intermixture, except slightly at the posterior part of the auricle, as already mentioned. From the right auricle it filled the right ventricle, passed along the pulmonary artery, and filled the *ductus arteriosus*, and branches going to the lungs. On entering the aorta it passed down that vessel, filling it completely without any intermixture of red; and thus all the branches of the thoracic and abdominal aorta were filled with yellow. The whole of the red had passed to the upper part of the body. In an experiment of this kind, the injection, after filling the left auricle, passes along the pulmonary veins, but during the life of the fœtus these veins must be filled by the blood returning from the pulmonary arteries. Had the injection been sufficiently minute, it would have passed from the pulmonary arteries into the pulmonary veins, and, consequently, we would have had some intermixture of yellow in the left auricle.

Though it may be doubted whether these two currents keep themselves so distinctly separate in the living fœtus as they appear to have done in this last experiment, yet, from the result of these three injections, we may be justified in concluding, that the blood returning from the placenta

principally passes to the head and superior extremities, and that the lower part of the body is principally supplied by the blood returning by the *cava superior*, or, in other words, by blood which has already made the circulation of the upper part of the body.

These injections of the fœtal heart and blood-vessels go to confirm the opinion which Sabatier, from a careful examination of the structure of the parts, entertained of the use of the Eustachian valve. The observations of Sabatier, although approved of and illustrated by Bichât,[1] seem scarcely to have satisfied the generality of anatomists and physiologists, that it could possibly prevent the intermixture of the two currents, in the manner he described, so that we frequently find this opinion referred to as the supposed use of the valve; and in two of the latest works on physiology, those of Magendie and Dr. Bostock, the use here assigned to it is, by the former, strongly stated to be impossible, and by the latter it is treated as a fanciful hypothesis.

The careful examination of the fœtal heart *in situ* must tend powerfully to remove the scepticism which usually exists on this subject. In the *first* place, after the right auricle has been laid open in a proper manner, we may observe that the *foramen ovale* does not exactly pass through the *septum* of the auricles, but is rather partly placed in a notch in the posterior and inferior part of the fleshy *septum*, and partly in the upper part of the *vena cava ascendens*, as it joins the superior *cava* to form the great *sinus venosus*. When we next examine the position of the Eustachian valve, running from the left of the entrance of the *vena cava inferior* to the left side of the *foramen ovale*, nearly forming a *septum* between the entrance of the vein and the right auricle, we perceive that this valve prevents the entrance of the blood of the *cava inferior*

[1] Anatomie Générale, tom. ii. Système Vasculaire. Article cinquième.

into the right auricle, and directs it through the *foramen ovale* into the left auricle. And lastly, when we consider the entrance of the two *cavæ* themselves, the superior passing downwards and forwards, the inferior upwards and backwards, and add to all this the thick upper margin of the *foramen ovale,* we further perceive that the blood passing down the *cava superior* must fall directly into the right auricle, to the left side of the Eustachian valve, and thus fill the right ventricle. The course which the currents take in passing along the *aorta* and pulmonary artery is confirmatory of injections of these arteries in the *fœtus*, made by Kilian.

The supposition that the superior nutrition of the head and upper extremities over the lower, may be accounted for by the greater purity of the blood sent to the upper part of the body, does not appear, when stated in this general manner, to be borne out by facts. For example, both the suprarenal capsules and kidneys are supplied by the blood that has already made the circulation of the upper part of the body: and yet we find the absolute size of the renal capsules and the comparative size of the kidneys, greater in the *fœtus* than in the adult. From the instances here adduced, it would appear that there is no necessity for having recourse to an arrangement of this kind, for regulating the growth of the different parts of the *fœtus*, notwithstanding that its circulating fluids are less pure than after birth, and that the usual method of augmenting the quantity of blood sent to the part, when its nutrition is to be increased, which we find adopted after the animal is enabled to maintain an independent existence, can apparently answer the same purpose during fœtal life.

To this, however, the brain may be an exception; and it is probable, that the peculiar arrangement of the circulation in the *fœtus*, may have a reference to the supply of that organ. It is perhaps necessary that an organ so delicate as

the brain, whose functions, at least in the adult, are so easily deranged by insufficiently aerated blood, should be supplied from a purer source than that which the two *cavæ* could furnish. For though the brain is in the fœtal state perhaps entirely a prospective organ, intended for an independent existence, yet we must suppose that its organization is from the first adapted to its independent state, and if this should be the case, it is highly probable that imperfectly aerated blood, which would be capable of deranging its functions, would be little favourable to its perfect development. It may be that this answers a similar purpose, though by different means, to the arrangements which G. St. Hilaire has described in the heart of the crocodile, the physiological condition of which approaches somewhat to that of the human *fœtus* during the later months of intra-uterine life, by which the brain and extremities are supplied with the best aerated blood, while blood less highly aerated is circulated among the other *viscera*.

An objection has been made to the application of these injections upon the dead *fœtus*, to explain the course of the blood in the living state. It has been said that an important element of the question has been omitted, by overlooking the influence of the vital contractions which circulate the blood. If, however, it be granted that the position of the Eustachian valve is such as is usually described, and that it presents an obstacle to the entrance of the blood passing along the *cava inferior*, into the right auricle, while it directs it toward the *foramen ovale* (which these injections prove,) it is plain that it must continue to do this, whether the fluid is set in motion by vital contractions, or by a syringe, more particularly as the auricles are quiescent during their filling.

I may here mention, that I am satisfied, from repeated dissections, that the usual description of the distribution of the umbilical vein of the *fœtus* through the liver is incor-

rect, and that the account of it given by Kilian, Dr. Knox, and probably by others, is the proper one. The umbilical vein, after passing along the horizontal fissure of the liver as far as the transverse fissure, enters the left branch of the *vena portarum;* and from this left branch of the *porta* generally directly opposite (though this varies a little) to the entrance of the umbilical vein, arises the *ductus venosus*, which passes backwards in the posterior part of the horizontal fissure, and enters one of the branches of the left hepatic veins before it joins the *cava ascendens.* If the umbilical vein be still described as dividing into two branches, it would be necessary to mention, that the branch which goes to the sinus of the *porta* supplies the left lobe of the liver; that it is never obliterated after birth, but afterwards becomes the left branch of the *vena portarum;* and, in fact, this is the view taken of it by Roux, in his additions to the *Anatomie Descriptive* of Bichât. Our notions regarding the circulation of the blood through the liver in the *fœtus* must remain the same whichsoever of these two descriptions we adopt.

[The most successful of these injections of the blood-vessels of the fœtus was presented by me to Professor Alison, by whom it was exhibited each session to his class, in his lectures on Physiology, as long as he taught this branch of medicine. Kilian[1] made his injection of the fœtal blood-vessels in the following manner:—He opened carefully both ventricles of the heart in a fresh still-born fœtus, and by means of two syringes threw at the same time two differently coloured injections along the aorta and the pulmonary artery. The injection thrown into the aorta filled the vessels supplying the head and thoracic extremities;

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[1] Ueber den Kreislauf des Blutes im Kinde, &c. S. 142. Karlsruhe, 1826.

that thrown into the pulmonary artery filled the descending aorta. The method which we followed not only illustrates the course of the two currents of blood propelled from the two ventricles, but also the office of the Eustachian valve. Kilian calls that part of the aorta extending from the left ventricle to the origin of the last of the large branches that supply the head and thoracic extremities, the *cerebral aorta;* and the pulmonary artery, the *ductus arteriosus*, and the descending aorta, (which three vessels form a continuous trunk,) he calls the *abdominal aorta.* According to this view there are two aortæ present in the fœtus, one of which, the cerebral aorta, springs from the left ventricle, and the other, the abdominal aorta, from the right ventricle. This view does not only rest upon the different courses followed by the two currents of blood sent from the two ventricles, but on the very diminished size of the arch of the aorta between the point where the left subclavian artery arises from it and its junction with the ductus arteriosus.]

No. X.

SENSATIONAL AND EMOTIONAL REFLEX ACTIONS.[1]

The muscles of the body are called into action in the healthy performance of their functions, in four different methods. 1. By direct excitation, and without the agency of the central organs of the nervous system, as in the case of the heart and the intestines. 2. By an impression made upon the afferent nerves, and conveyed inwards to the spinal cord, followed by the transmission outwards to the muscles to be moved, of a motive influence along the efferent nerves. These muscular movements last mentioned have been termed excito-motory. 3. By the excitation of certain sensations and emotions. 4. By the mental act of volition.

Though nearly all of the excito-motory muscular movements, as has already been explained, are, in the natural state of the body, preceded and attended by peculiar sensations, and though nearly all of them may be influenced to a certain extent by volition, yet there are good grounds for believing that the intervention of a sensation is not a

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[1] As I have had occasion to refer more than once in previous parts of this volume to sensational and emotional muscular movements, I have here introduced some observations on these, forming part of a lecture on the nervous system delivered several years ago.

condition necessary for the production of these movements; and when the excitations to which they respond are of a certain intensity, many of them are produced in spite of every voluntary effort to restrain them. The excito-motory movements require for their manifestation the presence of the spinal cord, and its encephalic prolongation upwards as far as the corpora quadrigemina or optic lobes, and the nerves attached to these. The two last kinds of muscular movements mentioned, viz. the emotional and sensational, and the voluntary, require on the other hand, as their designations imply, some mental act, and consequently the agency of the encephalon, for their performance.

I shall now make some remarks on the *sensational and emotional muscular movements.* Various associated muscular movements are linked with the excitation of certain sensations and mental emotions, all of which are arrested when the functions of the encephalon are suspended. These sensations and mental emotions may be excited by impressions made upon particular nerves, and conveyed to the encephalon, or they may arise from the mind recalling sensations and emotions formerly felt, and in both cases, as far as we can judge, the same efferent nerves are engaged in conveying outwards the motive influence to the muscles to be moved, as in the more simple excito-motory movements.

When the sensation of nausea is excited in the brain, either by the recollection of some disgusting object, by impressions made upon the optic or olfactory nerves, by disagreeable sights or smells, by emetic substances injected into the veins, by tickling the fauces, or by various deranged states of the abdominal organs, a motive influence is transmitted outwards along certain efferent or motor nerves to the various muscles—chiefly the diaphragm and abdominal muscles—whose combined contractions produce the action of vomiting. From whatever

cause the sensation of nausea originates, when it reaches a certain intensity, vomiting is produced. The associated movements of the numerous muscles of the chest and face, producing the short, rapid, and frequent expirations and peculiar facial expression which constitute laughter, may be induced by tickling the soles of the feet, the flanks, the arm-pits, and other parts of the body; by the consciousness that this tickling is about to be repeated; and also by a sense of the ludicrous arising in the mind, and by mirthful emotions. In all these different methods of calling forth the associated muscular movements of laughter, whether from impressions made upon the sensiferous nerves, and conveyed inwards to the encephalon, or from mental operations independent at the time of external impressions, certain mental acts must precede the transmission of the motive influence outwards along the efferent nerves to the muscles to be moved. When the sensation of yawning is induced, either by impressions made at the lungs, by the principle of imitation, or by organic causes developed in the constitution generally, a motive influence is transmitted outwards, along certain motor nerves, and a full and prolonged inspiration is made, during which the lower jaw is depressed, and a prolonged expiration immediately follows. Pandiculation, or stretching of the limbs, is also frequently induced by a sensation similar to that which leads to yawning. The short convulsive contractions of the muscles of expiration, but chiefly of the diaphragm, producing hiccup, are in all probability dependent upon sensations excited by impressions made upon the gastric branches of the vagus.

When any substance is thrust near the eye, the orbicularis palpebrarum muscle is instantly closed, though the substance has never touched any part of the body. No sooner is the sensation of a body approaching the eye experienced, than a motive influence is transmitted along

some of the fibres of the portio dura nerve, and the orbicularis palpebrarum muscle is thrown into contraction without the agency of volition. When the sensation of pain is felt in great intensity from impressions made upon the sensiferous nerves being conveyed to the encephalon, or from changes in the encephalon itself, a motive influence may be transmitted outwards, along various efferent nerves, and the jaws are firmly compressed or gnashed together, and the orbicularis oris muscle contracted; and when the sensation of pain is very intense, the chest is inflated as in full inspiration, the sides of the mouth are drawn outwards exposing the teeth, the alæ of the nose are widely separated, the forehead is corrugated, the eyelids raised, and various muscles of the limbs and trunk thrown into contraction, producing writhing of the body. The motive influence may also during the excitation of the sensation of pain, be sent along the efferent nerves of the muscles of the larynx and chest, and cause at one time groaning, and at another that instinctive cry indicating suffering, and which is the same in all the individuals of each species of animals. The sensation of cold when of sufficient intensity, is followed by the short irregular contractions of the voluntary muscles which produces trembling. When this arises from external causes, the impressions are made, as in other cases of this kind we have mentioned, upon the sensiferous nerves, by the abstraction of part of the caloric from the surface of the body, and conveyed upwards to the cerebrum, where they excite the sensation of cold, and the motive influence is then transmitted outwards along the efferent nerves to the muscles to be moved. The sensations which precede and are connected with the completion of the venereal act, may be attended by panting and convulsive movements of the muscles of the trunk and limbs. Some sensations exert a considerable influence upon the involuntary muscles, especially the heart. The sensation of

pain accompanying peritonitis, quickens and greatly enfeebles the contractions of the heart, and it is believed that the great advantage of opium in this disease arises from its abating the intensity of this sensation.

The various mental emotions and passions which agitate, delight, and afflict the human race, are attended by involuntary movements of the muscles of the body, but particularly of those of the face, eyeball, chest, and larynx, which may convey to a bystander a more vivid conception of the operations going on within the mind of an individual than could be conveyed to him by words. The operations of the intellectual faculties are, also, in general each revealed by its own peculiar facial expression; the features assume an expression of deep thought, of imaginative musing, or of vacancy. These involuntary muscular movements have been justly termed the natural language of expression. The influence of the mental emotions upon the movements of the heart are so familiar to every individual, that in common language that organ is regarded as the seat of many of the emotions and passions, and this idea has had a powerful influence upon the phraseology of all languages. As the number and extent of these involuntary muscular movements are in proportion to the psychological endowments of the animal, it is only in man that they are observed in their fullest extent. In those individuals of the human species, who are unaccustomed to control by an act of volition those outward indications of the emotions and passions, each of them is manifested by its own peculiar combination of the contractions of the muscles of the face, forming the various kinds of facial expression. No one can fail readily to detect and correctly interpret the facial expressions of joy, grief, rage, love and hatred, anguish, horror and despair, veneration and awe, when strongly marked. The muscles of the face principally concerned in producing these various effects,

are those which move the angles of the mouth and the eyebrows. The artist may by a change in the elevation or depression of the angles of the mouth, and a slight alteration upon the inner angle of the eyebrows, convert at pleasure the facial expression into that of mirth or grief. The efferent nerve chiefly engaged in producing these varied combinations of the muscles of the face is the portio dura. The elevation of the upper eyelids, and the motions of the eyeball, depend upon the third, fourth, and sixth pairs of cerebral nerves; and the elevation of the lower jaw upon the non-ganglionic portion of the fifth.

Though the involuntary movements of the muscles of the face are, in the human species, those chiefly engaged in the natural language of expression, yet those of the respiratory muscles of the chest and larynx are also sufficiently varied and characteristic; and in the lower animals principally serve to express their wants and their limited number of mental emotions. When the emotion of grief is intense, a motive influence may be sent along the efferent nerves, and excite those associated movements of the respiratory muscles which constitute sobbing and weeping. When mirthful emotions arise in the mind, the respiratory muscles are stimulated to another kind of associated action, as in laughter. The muscles of the larynx act more especially in combination with the respiratory muscles of the chest in producing the instinctive cries of joy, anguish, horror, and despair. The panting of terror, and the full inflation of the chest in rage, manifest the effects of some of the other emotions and passions upon the respiratory movements. The other muscles of the trunk, and those of the limbs, may be also called into various associated involuntary movements by the mental emotions and passions, constituting the involuntary attitudes and gestures. The involuntary start of surprise, the muscular tremblings and relaxation of terror, the leap of ecstatic joy,

the clenched fist in rage, the shrugging of the shoulders in impatience, the tossing of the head in contempt, the position of the body when the mind is strongly impressed by veneration or awe, all illustrate the influence of the mental emotions and passions in exciting certain determinate involuntary movements of the muscles of the limbs and trunk when not kept under control by a strong effort of volition.

That these muscular movements are not voluntary, though capable of being controlled by volition, is sufficiently proved by the fact, that they are common to all individuals of the human species, however widely apart they may be placed, they are manifested independently of all education, and they are instinctively interpreted by others. If the involuntary emotional muscular movements can be controlled by volition, the mental emotions and passions may, on the other hand, influence the contractions of the voluntary muscles. The voluntary muscular movements are enfeebled in terror and are vigorous in rage. The emotions and passions also exert a powerful influence upon some of the involuntary muscles, as upon the heart, intestines, uterus, and bladder. The heart's action is increased, and is said to leap from joy, it palpitates from anxiety, becomes irregular, and even sometimes is entirely arrested from terror, and is depressed by grief and despair. The effects of mental emotions and passions upon the heart are more strongly manifested in women and children and during an enfeebled state of the body. The contents of the bladder and rectum may be involuntarily expelled during the emotions of terror, especially in children. The sensations, the mental emotions and passions, also exert important effects upon the capillary circulation and the secretions, independently of their influence upon the muscular movements of the heart. The effects of shame in producing the sudden suffusion of the face in blushing, the instantaneous paleness of the face and the abundant secretion of sweat in terror, the flushing of

the forehead in anger, the increased secretion of tears and the diminished secretion of saliva from grief, the effects of the sensation of pain and of the depressing passions of the mind in diminishing, vitiating, or arresting the secretion of the gastric juice, the increased secretion of seminal fluid in the testes, and of an increased quantity of mucus in the vagina, from certain sensations and animal desires, and the increased secretion of milk in the mamma, from the pleasing emotions that arise in the mind of the mother as she looks upon her offspring, all illustrate the effects of the sensations, the mental emotions and passions upon the capillary circulation and the secretions. In all these cases the effects upon the capillary circulation and the secretions are produced by an agency sent outwards from the encephalon, along the nerves distributed in the structures whose functions are thus influenced, but the efferent nerves which convey this are not the motor encephalic nerves and the anterior roots of the spinal nerves, but the sensiferous encephalic nerves and the sympathetic, and probably also the posterior roots of the spinal nerves.

The influence exerted by these mental acts upon the action of the heart, the capillary circulation, and the secretions, especially those connected with the digestive process, is of very considerable importance in a pathological point of view, as it may either enable the body to resist, or render it more liable to the attacks of disease. The emotions of hope and joy, and the agreeable sensations when not excessive and transient, but more moderate and steady, promote the capillary circulation in the surface of the body and the elimination of the necessary secretions, and thus render the body capable of withstanding the causes which excite disease, and of resisting it when once formed. On the other hand, the depressing passions, such as grief, anguish, and despair, by enfeebling the capillary circulation, and diminishing or vitiating the secretions, favour the agency

of the causes which induce disease, and impede the operations of these actions by which the body may rid itself of its maladies. The effects of the exciting mental emotions in promoting health, and of the depressing passions in inducing disease, have been frequently exemplified upon large bodies of individuals, and are best illustrated by the history of armies. It is not unusual to find an army, when flushed with victory and elated with hope, maintain a comparative immunity from disease under physical privations and sufferings, which under the opposite circumstances of defeat and despair produce the most frightful ravages.

No. XI.

ON SOME POINTS IN THE ANATOMY AND PHYSIOLOGY OF THE HEART.

(FROM THE ARTICLE HEART IN THE CYCLOPÆDIA OF ANATOMY AND PHYSIOLOGY, WRITTEN IN 1836.)

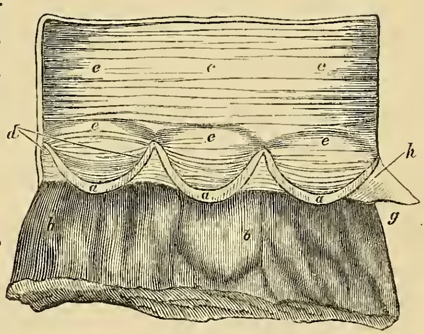
Tendinous texture.—The tendinous texture of the heart is placed—1, around the auriculo-ventricular and arterial orifices; 2, within the reduplicature of the lining membrane, forming the auriculo-ventricular and arterial valves; 3, it forms the cordæ tendineæ.

Auriculo-ventricular tendinous rings. — Around each auriculo-ventricular opening we find a tendinous circle or ring, from the upper part of which the muscular fibres of the auricles arise, and from the lower part those of the ventricles, thus affording perhaps the only example in the human body of a strictly involuntary muscle having tendinous attachments. The tendinous ring surrounding the left auriculo-ventricular opening is stronger than that surrounding the right. These tendinous *zones* are thicker along the lower edge where the muscular fibres of the ventricle are attached, and become thinner along the upper edge where the muscular fibres of the auricles are attached, so that the fat occupying the auricular groove is seen through the upper portion of the ring on the right side. The right margin of the left auriculo-ventricular ring is connected with that surrounding the aortic opening. The

existence of the auriculo-ventricular and arterial tendinous rings was well known to Lower.[1]

Arterial tendinous rings.—The form of the tendinous rings surrounding the arterial openings, and the manner in which the large arteries are attached to their upper edges, have not, I think, been described with sufficient accuracy. These textures are very plainly observed in the heart of the ox and horse after a little dissection. The following description is drawn up from numerous dissections of these parts made on the human heart. The tendinous ring surrounding the aortic opening is stronger and thicker than that surrounding the orifice of the pulmonary artery. Both of them are stronger than the auriculo-ventricular rings. Each of the arterial rings appears as if composed of three semilunar portions placed on the same plane, the convexities of which are turned towards the ventricles, and the concavities towards the vessels. (*a a.*)

Each of these semilunar portions has its projecting extremities intimately blended at their terminations with the corresponding projecting extremities of those next to it, so that the three form a complete circle, with



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three triangular portions projecting from its upper edge.

[1] Tractatus De Corde, p. 29. 1669.

[2] Representation of pulmonary artery slit open, and dissected so as to show the three tendinous festoons at its origin; and the attachment of the middle coat of the pulmonary artery, and some of the muscular fibres of the ventricle to these. In slitting open the pulmonary artery one of the three projecting extremities of the tendinous ring has been divided. *h* Remains of one of the sigmoid valves attached to one of the tendinous festoons.

The semilunar portions approach fibro-cartilage in their structure, and have the intervals left between their convex edges filled with a texture more decidedly fibrous (*d*), and which is considerably weaker than the semilunar portions, more particularly on the left side of the heart.[1]

The thinness of the tendinous structure filling up these intervals, has led some anatomists erroneously to describe these portions of the heart as protected only by the two serous membranes. The right tendinous zone is broader than the left and very thin, particularly at its inner margin, at which part in both sides of the heart it assumes more of the tendinous than of the fibro-cartilaginous structure. These tendinous rings are placed obliquely from without inwards, and from above downwards, so that the outer edge is placed on a plane superior to the inner. The sigmoid valves are attached to the inner edge of the upper surface (*h*), and the tendinous fibres placed in the fixed margins of these valves contribute to the thickening of the ring at this part. The middle coat of the arteries is connected to the outer edge of the same surface, and to the anterior part of the projecting extremities of the festoons (*e*), while the muscular fibres of the ventricles (*b b*) are attached to the lower surface of their more prominent convex portions, and to the lower margin of the fibrous tissue, filling up the space between them (*d*). There is, however, this difference between the right and left arterial openings, with respect to the attachment of the muscular fibres; on the right side the muscular fibres arise from the convexity of the whole three tendinous festoons, while in the left side the muscular fibres are attached only to one and part of a second, as the larger lip of the mitral valve is suspended from the posterior or left, and a great part of the anterior, in

[1] These intervals are occupied by muscular fibres in the heart of the ox and horse.

fact to that part of the tendinous ring, which separates the aortic from the auriculo-ventricular opening. From the posterior part of that portion of the tendinous ring to which the mitral valve is connected, the anterior fibres of both auricles near the septum arise. As the left tendinous ring is thicker and narrower than the right, there is a larger space left between the fixed edge of the valves and the attachment of the middle coat of the arteries, on the right than on the left side. This space is of some importance, as upon it a considerable part of the pressure of the column of blood in the large arteries must be thrown during the diastole of the ventricles.

There is a good representation of these tendinous rings given in Table II. Opera Valsalvae, tom. i. At page 129 they are thus described:—"In horum sinuum ambitu quà valvulvæ sinubus annectuntur quidem quasi agger videtur occurrere substantiæ durioris ad similitudinem cartilaginis tarsi palpebrarum." I find also that Gerdy[1] appears to have had an accurate notion of the form and appearance of these tendinous rings. He was aware of the existence of the projecting angles of the tendinous ring which pass up between the festoons of the middle coat of the arteries, and which have been overlooked in succeeding descriptions. I find also that the late Dr. A. Duncan, jun., has in his unpublished manuscript, given a very accurate account of these structures in the heart of the ox.

Tendinous Structure in the Auriculo-ventricular Valves.—Distinct tendinous fibres exist in the auriculo-ventricular valves enclosed between the reduplication of the lining serous membrane. These are continuous with the auriculo-ventricular tendinous zones, and are most distinct and of great strength at the base. I could never observe any distinct traces of muscular fibres in these valves in the human

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[1] Journal Complémentaire, tom. x.

heart, either when fresh or after long boiling. Bouillaud has, from the examination of one inconclusive case, but principally from analogy with the corresponding valves of the heart of the ox, supposed that they may exist in some cases in hypertrophy of the valves. In making examinations of this kind we must be exceedingly careful not to mistake the tendinous fibres when tinged with blood for muscular fibres, for under these circumstances they certainly at all times assume to the naked eye the appearance of muscular fibres.[1]

Tendinous Structure in the Arterial Valves.—Distinct tendinous fibres also exist in the arterial valves, which must add considerably to their strength, and prevent their more frequent rupture. Three of these tendinous bands in each valve are stronger than the others, and their position deserves attention, as they are often the seat of disease. One of these bands occupies the free margin of the valve, and passes between the projecting extremities of the tendinous festoons. Upon the middle of this band the corpus Arantii, which is formed of a similar texture, is placed. The other band comes from a point a little above the projecting end of the tendinous festoon, and passes up in a curved manner towards the corpus Arantii, leaving between it and the superior band, a triangular space on each side, in which, if any tendinous fibres exist, they are exceedingly obscure. These two tendinous bands were little known to Morgagni. The third band is placed in the attached margin of the

[1] In the heart of the dog I have seen a distinct band of transverse muscular fibres in the base of the lower lip of the mitral valve, but could never satisfy myself of the existence of any longitudinal muscular fibres. In the heart of the ox and horse very distinct longitudinal muscular fibres are seen in these valves on both sides of the heart, principally, if not entirely, continuous with the inner layer of the fibres of the auricles. A greater part pass over the inner surface of the tendinous rings, and are firmly attached to the tendinous structure of the valves, reaching nearly to the lower margin of the smaller segments of the valves. The effect of these fibres upon the movements of the valves would form an interesting subject of investigation.

valve, and renders this part the thickest and strongest. Between the middle band and the attached margin of the valve a number of weaker bands are placed, which also pass upwards, generally assuming a curved form. Morgagni termed these lower and weaker fibres *fibræ carneæ*, but they evidently belong to the same structure as the stronger bands. The arrangement of these tendinous fibres is best seen in the aortic valves, and have been well represented by Morgagni.[1]

Attachment of the middle coat of the arteries to the arterial tendinous rings.—The inner and outer serous membranes are continued from the heart upon the arteries, the one becoming the inner coat of the arteries, and the other is continued for a short distance upon their external surface. A thin layer of cellular tissue also passes from the heart along the arteries, between their middle coat and their external serous membrane. These are, however, so far unimportant compared with the attachment of the middle coat of the arteries to the tendinous festoons which we have just described. The middle coat is so very firmly and strongly attached both to the external edges and to the anterior portion of the upper part of these projecting extremities (*e*) that it can be detached with great difficulty. Those fibres of the middle coat attached to the projecting extremities which are apparently of the same number and thickness as in that portion of the artery immediately above, form a distinct curved edge (*e*) as they pass from the extremity of one festoon to the other. As we trace the middle coat of the artery downwards into the concavities formed by each festoon we find that below this curved edge they become strikingly thinner, and continue to diminish in thickness and in length (since they can only stretch between the projecting extremities) until we arrive at the bottom of the concavity.

[1] Adversaria Anatomica, Tab. IV. Fig. 3.

These three thin portions of the middle coat must then be placed behind the semilunar valves, and correspond to the sinuses of Valsalva.[1]

The thinness of the middle coat at the sinuses of Valsalva will render this portion of the artery more dilatable, and predispose it to rupture when its coats are diseased.[2] The tendinous zones are distensible, but to a considerably less extent than the middle coat of the arteries. I am not aware that this account of the manner in which the middle coat of the arteries is attached to the tendinous rings has been previously given. I suspect, however, that Dr. Duncan must have been perfectly aware of it from some parts of his manuscript. The differences between these tendinous festoons and the yellow elastic coat of the arteries, and the manner of their attachment, can easily be made out in the human heart; they are, however, more apparent in the larger animals, as the horse and the ox. The different characters of the two tissues are obvious at the first glance after boiling, even in the human heart.[3]

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[1] So striking is the difference between the middle coat as it fills up the concavity of these festoons, and where it stretches between the projecting extremities in the hedgehog, that at first sight it appears to be deficient at that part.

[2] According to Valsalva aneurisms are frequently found in this situation, " Atque hic aortæ sinus maximus ille est, in quo sæpe aneurysmata circa præcordia contingunt, ut propria observatione edoctus sum."—Valsalvæ Opera, Epist. Anat. ed. Morgagni, tom. i., p. 131. 1740.

This greater tendency to aneurismatic dilatation must depend upon two circumstances. The increased calibre of the artery at this part will increase the pressure upon its walls from the well known hydrostatic law, that " in a quantity of fluid submitted to compression, the whole mass is equally affected, and similarly in all directions," and the diminished thickness of the middle coat will materially favour this distending force.

[[3] The article in the Cyclopædia of Anatomy and Physiology from which the above is copied, contains a very considerable number of errors, which could not have escaped my notice had I corrected the press. A proof of the article was sent me when ill of typhus fever, and it was returned without any intimation of its not being corrected. Among several other errors it is stated that the septum of the ventricles is convex towards the left ventricle, *instead* of towards the right ventricle.]

Mode of action of the valves of the heart.—While the blood is rushing through the auriculo-ventricular openings during the contraction of the auricles, the lips of the mitral and tricuspid valves are separated from each other and thrown outwards from the axes of these openings, and the larger lip of both is at this time carried towards the arterial orifices. It has generally been supposed that the mitral and tricuspid valves are, during the systole of the ventricles, passively floated up towards, and obstruct the auriculo-ventricular orifices so as to prevent the free regurgitation of the blood into the auricles; and that the use of the cordæ tendineæ is merely to limit the movements of the valves—to permit them to be raised sufficiently to close the orifices, but at the same time to provide against the otherwise unavoidably fatal consequences that would result from their being carried through into the auricles by the current of blood. Mayo,[1] Bouillaud,[2] and others, have however maintained that the lips of these valves are not approximated in the mechanical manner just stated, but by the contraction of the musculi papillares, of which the cordæ tendineæ are the proper tendons. As the musculi papillares contract along with the other fibres of the ventricles, the lips of each of these valves are drawn towards the axis of the opening, and are closely applied to each other, forming a kind of cone, the apex of which projects downwards into the ventricles. It is from the adoption of these views that Bouillaud proposes to call these musculi papillares, the tensor, elevator, or adductor muscles of the valves. That the lips of the valves are approximated in this manner appears to me to be the much more probable opinion; for when we examine the uniform position and course of the musculi papillares and cordæ tendineæ, more particularly those of

[1] Outlines of Physiology. 2d edition. 1829.
[2] Traité Clinique des Maladies du Cœur, tom. i. 1835.

the left ventricle, that the cordæ tendineæ pass from each musculus papillaris to both lips of the mitral valve, occasionally crossing each other; and that the posterior or smaller lip, though it may be drawn inwards so as to meet the larger and more moveable, is so bound down as to be scarcely capable in most cases of being floated up on a level with the orifice; and further, when we also remember that the musculi papillares contract at the same time with the other fibres of the heart, we can scarcely resist coming to this conclusion. Besides, if the lips of the valves were floated up to the orifice, a greater quantity of blood would regurgitate into the auricles during the systole of the ventricle than in all likelihood takes place; for as the lips of the valve must be widely separated from each other when the systole commences, it is evident that a less quantity of blood must have passed through the orifice before the lips are sufficiently approximated to obstruct its further passage when these are assisted by an active force, than when they are merely passively brought together by the current of blood passing in that direction. It has, however, been supposed that the musculi papillares do not contract with the other fibres of the ventricles. Haller states[1] that on laying open the heart he has seen the muscles of the valves contract during the systole of the heart. It may be objected to this experiment that the unusual stimulus applied to the heart in cutting its fibres across may have deranged the usual order of its contractions. I have repeatedly opened the heart in rabbits and waited until its contractions had ceased, and on renewing its movements by irritating the inner surface at a distance from the cut edges, I have observed that the columnæ carneæ acted simultaneously with

[1] Elementa Physiolog. tom. i., p. 390. Sur le Mouvement du Sang. Mémoires sur la Nature sensible, tom. i., p. 379.

the other muscular fibres of the heart.[1] I was also satisfied that the musculi papillares were proportionally more shortened during their contraction than the heart itself taken as a whole, which is nothing more than what we would expect when we remember that the fibres of the musculi papillares are so far free and run longitudinally, while by far the greater part of the other fibres run in a spiral manner.

Haller, in relating his observations on the contraction of the musculi papillares, makes another statement, which, however, is decidedly adverse to this opinion. The cordæ tendineæ appeared to him to be relaxed during the contraction of the musculi papillares.[2]

It is difficult to make satisfactory observations upon the effects of the contractions of the musculi papillares upon the tension of the cordæ tendineæ. In several animals upon which we attempted to ascertain this, it was only when the heart was acting languidly that we could observe what was likely to be the effect of the contraction of the musculi papillares on the cordæ tendineæ when they were placed as far as possible in their natural relation to each other. We could never observe that they contracted sufficiently to move the valves, but they certainly rendered some of the cordæ tendineæ more tense. When, however, we take into account, that in an experiment of this kind the valves are not thrown out widely from the orifices of the auriculo-ventricular orifices, the ventricle is not distended with blood, the cordæ tendineæ consequently not put so far on the stretch as occurs at the commencement of the systole, and that the contractions of the musculi papillares are languid,

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[1] [The observations of the London Committee appointed by the British Association to examine into the motions and sounds of the heart confirmed this view of the simultaneity of contraction of the columnæ carneæ and ventricular fibres.—Note by the Editor of the Cyclopædia of Anatomy and Physiology.]

[2] Sur le Mouvement du Sang, pp. 125, 126. 1756.

we can easily perceive how, in the natural systole of the heart, these contractions of the musculi papillares should be sufficient to move the valves inwards, though not to such an extent as to apply them closely to each other. The contraction of these musculi papillares apparently sets the valves in motion, and they are subsequently applied to each other by the currents of blood. It may be supposed that if the contraction of these musculi papillares can render the cordæ tendineæ sufficiently tense to move the valves, this would prevent the subsequent elevation of them to obstruct the auriculo-ventricular opening. We believe, however, that it is only at the commencement of the systole that they are sufficiently tense to move the valves, for as the contraction proceeds the capacity of the heart is so much diminished, both in its transverse and longitudinal dimensions, that they become relaxed. Besides, if we could suppose that these musculi papillares are capable of contracting through a sufficient space to draw the valves together, this would be all that is necessary to prevent the regurgitation of the blood through the auriculo-ventricular opening.

So convinced, indeed, were the older anatomists and physiologists that the cordæ tendineæ are relaxed during the systole of the heart, and of the necessity of an accompanying diminution of the length of the ventricles themselves to effect this, that this argument adduced by Bassuel appears to have been principally instrumental in deciding the once keenly controverted question whether or not the heart was elongated during its contraction.

It may be supposed that the relative size of the auriculo-ventricular orifices to the length of the lips of the valves would not admit of their apices being brought together in the form of a cone as described, but it must be remembered that from the course of the muscular fibres in the immediate neighbourhood of those openings, their areas must be diminished during the systole of the heart. There is at

least one thing certain connected with the action of these valves, viz., that the contraction of the musculi papillares can never cause the valves to strike the inner surface of the ventricle and produce a sound, as has been supposed.

The manner in which the semilunar valves at the origin of the aorta and pulmonary artery perform their office is entirely mechanical and easily understood. During the systole of the heart they are thrown outwards from the axes of these vessels; but during its diastole, when part of the blood driven into the artery would fall back into the ventricles, these valves are thrown inwards and obstruct completely the whole calibre of the arteries. In all probability the sinuses of Valsalva placed behind these valves contain a certain quantity of blood even during the systole of the heart, and this reacting upon the valves through the agency of the elasticity of the arteries brought into operation at the termination of the systole, materially assists in producing the more rapid and certain action of the valves.[1]

Impulse of the heart.—It has been at various times, and still is by some late and modern experimenters,[2] maintained that the apex of the heart strikes the parietes of the thorax during its diastole, and not during its systole. This is in reality what we would *à priori* expect, for it certainly does at first appear somewhat paradoxical that the heart should strike the parietes of the chest when the apex is approxi-

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[1] [I believe that the use here assigned to the sinuses of Valsalva is one of considerable importance, and effects the more certain and rapid approximation of the semilunar valves. The blood contained in the sinuses of Valsalva is set in motion during the diastole of the ventricles, by the elastic walls of the arteries, and the free margins of the semilunar valves thus pushed slightly inwards and apart from the inner wall of the arteries to which they were closely approximated during the systole of the ventricles, are readily caught by the descending currents of blood.]

[2] Pigeaux, Stokes, Burdach, and Beau. Dr. Corrigan has, much to his credit, publicly renounced his previously published opinions on this question, after more accurate observations had convinced him in his error.

mated to the base. The concurrent testimony of the most accurate observers has, however, fully established the correctness of the fact. Harvey observed it in the human body where the heart had been exposed from the effects of disease.[1] One of the principal arguments adduced in support of this opinion by these authors was drawn from the fact that the pulse at the wrist is not synchronous with the impulse against the chest—an opinion which had been pretty generally maintained since the time of Aristotle. It is difficult to be convinced of this when the pulse is quick; but when it is slow, and in certain cases of disease of the heart, it can generally be satisfactorily ascertained. So far then they are right, but in the next and most important step of the argument they fall into a decided error; for they proceed upon the supposition that the pulse is synchronous in all the arteries of the body at the same time, and consequently the impulse of the heart at the chest cannot be synchronous with the flow of blood along the arteries, or, in other words, with the systole of the heart. In opposition to this opinion, Dr. Young[2] had previously shown, upon the principles of hydraulics, that the pulse along the arteries must be progressive, yet in general so rapid as to appear to arrive at the extremities of the body without the intervention of any perceptible interval of time. And when the attention of medical men was turned to this subject, various observers soon ascertained by repeated experiments that the pulse could be felt in favourable cases to pass along the arteries in a progressive manner—that the pulse

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[1] "Simul, cordis ipsius motum observavimus, nempe, illud in diastole introrsum subduci et retrahi; in systole vero emergere denuo et protrudi: fierique in corde systolem quo tempore diastole in carpo percipiebatur: atque proprium cordis motum et functionem esse systolem: denique cor tunc pectus ferire, et prominulum esse; cum erigitur sursum, et in se contrahitur." Exercitationes de Generatione Animalium, pp. 313, 314. Amstel. 1651.

[2] Phil. Trans., p. 1. 1809.

in the large arteries at the root of the neck and impulse at the chest are synchronous or nearly so, that both precede that at the wrist, and more distinctly still that of the dorsal artery of the foot.[1]

Various attempts have been made to explain in what manner the apex of the heart is made to impinge against the parietes of the chest by those who maintain that it occurs during the systole of the ventricles. Senac supposed that this was principally effected by the curvature of the two large arteries, but principally of the aorta, which arise from the ventricles; for at each stroke of the ventricles when an additional quantity of blood is driven into the large arteries, as they are curved they make an attempt to straighten themselves; and as this takes place to a slight extent, the heart, which is attached to their extremities, ought to be displaced, and its apex, which describes the arc of a circle greater than the other parts of the heart, is thus made to impinge against the walls of the chest. He also believed that the distention of the left auricle with blood during its diastole has also, from its position between the spine and base of the heart, the effect of pushing the heart forwards; and this occurring at the same time with the attempt which the curved arteries make to straighten themselves, it thus acts as a second or subsidiary cause in tilting the heart forwards.[2] Though this supposed effect of the curvature

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[1] It is interesting and curious, as shewing the revolution of opinions, to compare the strict similarity of the arguments adduced by the modern supporters of this doctrine with those maintained by Shebeare in 1755. (Practice of Physic, vol. i., p. 193.) "This, however plausible it may appear, cannot be the true cause of it (impulse of the heart,) because then this stroke must be during the systole of the ventricles, which would be synchronous with the diastole of the arteries; whereas the beating of the heart precedes the dilatation of the arteries, and thence this stroke must be made during the diastole of the ventricles: thus the diastole or distention of the heart is the cause of the beating against the ribs."

[2] Op. cit., tom. i., p. 356. The cause of the tilting motion of the heart was also, at a later period, attributed to the curvature of the aorta, and to

of the large arteries has been a favourite explanation with many of the impulse of the heart against the chest, yet it really appears to have little, if any, influence in producing this. Shebeare,[1] and, more lately, Dr. Corrigan,[2] have shown that the direction of the curvature of the large arteries is such, that if any effect of this kind is produced, the heart would not be carried to the left side, but in the direction of the curve, which is exactly in the opposite direction. Besides, the tilting forwards of the heart has been observed though no blood was passing along the large vessels at the time, and the same thing takes place after the large vessels have been cut through, and the heart removed from the body.[3] Haller and others have supposed that the secondary cause assigned by Senac, viz., the sudden distention with blood of the left sinus venosus which lies impacted between the spine and left ventricle—is the principal if not the sole cause by which the heart is pushed forwards against the ribs. In confirmation of this opinion Haller states,[4] that if we inflate the left auricle after having opened the chest, we see the point of the heart approach with vivacity the region of the mamma. As we cannot, however, under these circumstances, distend the auricle without also distending the corresponding ventricle, this movement of the heart depends more upon the sudden inflation of the ventricle than upon any distention of the auricle, as any one may easily satisfy himself by repeating the experiment. Besides, the distention of the auricles by the blood flowing along the veins

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this exclusively by Dr. W. Hunter. Note in John Hunter's Treatise on Inflammation, p. 146. 1794.

[1] Op. cit., p. 195.

[2] Dublin Med. Trans., vol. i., p. 154. 1830.

[3] Dr. Carson (Inquiry into the Causes of the Motion of the Blood, p. 183,) maintains that no proof can be adduced that the curvature of the aorta is rendered more straight during the systole of the heart.

[4] Sur le Mouvement du Sang, pp. 129, 130. 1756.

is too gradual for this sudden and rapid impulse of the heart; nay, more, the impulse may be observed when no blood is flowing into the auricles. Sabatier[1] believed that this impulse depends upon two causes: 1st, principally upon the distention of the auricles, more particularly the left; and, 2dly, upon the curvature of the large arteries. Apparently, however, perceiving the necessity of there being a sudden distention of the auricles to produce this, he supposed that this was effected by the auriculo-ventricular valves. He argued that as these valves during the diastole of the heart form a cone stretching from the base towards the point of the ventricle, which is full of blood when the systole commences, when the valves are carried upwards to obstruct the auriculo-ventricular orifices, this blood is pushed before them into the auricles, producing a reflux into the auricles, and this with the blood flowing along the cavæ and pulmonary veins, causes a sudden distention of the auricles, which pushes the ventricle forwards.[2] Meckel appears to have adopted the opinions of Sabatier. We need not repeat our objections to this explanation. Dr. Alison, perceiving the insufficiency of all these explanations, has for a considerable time past suggested in his lectures, that this might be explained by the arrangement of the fibres, "more particularly by the irregular cone which they form being *flattened posteriorly*, and by the consequent greater mass of fibres on the anterior surface." More lately Mr. Carlisle[3] has also attempted to explain this by the greater length of the anterior fibres of the heart than of the posterior. As the shape of the ventricles is an oblique cone, and as they have their longest sides in front, he argues, "that it is a law of muscular

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[1] Traité complet d'Anatomie, tom. ii., p. 230.

[2] Dr. Bostock has failed of his usual accuracy in detailing the opinion of Sabatier on this question.

[3] Transactions of British Scientific Association, vol. iii. Dublin Journal of Medical Science, vol. iv.

contraction that fibres are shortened during their contraction in proportion to their length when relaxed. For instance, if a fibre one inch long lose by contraction one-fourth of its length, or one quarter of an inch, a fibre two inches in length will lose one inch by contractions of equal intensity. The apex then does not approach the base in the line of the axis of the ventricles, but is drawn more to the side of the longer fibres, that is, towards the front, thus producing the tilting forwards." We believe that it may be proved on mechanical principles, that though the anterior and left surfaces of the ventricles are considerably longer than those on the posterior and right, yet during their contraction, when they are drawn towards their fixed attachments, if the fibres are of equal thickness, the apex will be drawn up nearly in the diagonal of the two forces, and that if any tilting upwards of the apex take place, this will be only to a small extent, and be quite insufficient to account for the impulse felt at the chest. We must therefore look to some other circumstances besides a mere difference in length of the two surfaces to account for this. Mr. Alderson[1] has ingeniously attempted to apply here the law of action and reaction between bodies—one of considerable importance in mechanical philosophy, and upon which Barker's centrifugal mill has been constructed. Unfortunately, however, for this explanation, the axes of the large arteries and the direction in which the apex is tilted do not by any means accord. Dr. Hope's supposition that "the retropulsion of the auricular valves" may assist in producing this impulse—"as these act on a column of blood which offers a greater resistance than the weight of the heart the action is reflected on the organ itself and impels it forwards"—is, on the other hand, completely opposed to the law that action and reaction are the same. As well may a man

[1] Quarterly Journal of Science, &c., vol. xviii., p. 223.

attempt to propel a boat by standing in the stern, and pushing with an oar against the prow. Dr. Filhos attributed the impulse to the spiral turns of the fibres at the apex of the heart attempting to straighten themselves during their contraction, and so raise themselves suddenly and throw themselves forwards. The objections to this explanation are so palpable that they must occur to every one. Since the tilting forwards of the apex of the heart is observed after the blood has ceased to flow through its cavities, it is obvious that we must look for the cause of this in the arrangement of the muscular fibres themselves, though it may be difficult to point out that particular arrangement. It appears to me that the distribution of some of the strong bands of fibres, the course of which I have already described[1] when treating of the muscular tissue of the heart, may satisfactorily account for it. We there pointed out that several strong bands of fibres arise from the base of the septum between the ventricles, pass downwards and form part of the septum, then emerge from the anterior longitudinal groove, and wind round in a spiral manner to form both the anterior and posterior part of the lower portion of the heart. On entering the apices of the ventricles, (principally the left,) the fibres are scattered over their inner surfaces, and while a great number of them go directly to be inserted into the tendinous rings, others form part of the columnæ carneæ. We have thus strong bands of fibres attached by one extremity (their septal extremity) to the base of the ventricles at a point pretty far posterior, while at the other extremity many of the fibres are loose, or at least only attached to the tendinous rings through the medium of the cordæ tendineæ and valves, which must admit of a certain degree of contraction of these fibres before they become tense. At each systole of the heart

[1] In Article *Heart* in the Cyclopædia of Anatomy and Physiology.

when these fibres act, it is evident that the tendinous rings must form the fixed points towards which all these fibres contract; and since they are by one extremity all closely and directly connected to a fixed attachment, viz. the tendinous rings, while by their other extremity part only are directly attached to the tendinous rings, the other part being loose, or at least only connected to the tendinous rings through the lax cordæ tendineæ and valves, it must follow that the force with which the contraction takes place towards the septal extremity must preponderate over the other. If these bands of fibres had been as closely connected to the tendinous rings at the one extremity as at the other, then the force of the contraction towards both would have been equal; but since this is not the case, the apex must be carried forwards at the same time that it is drawn upwards towards the base. This forward motion may also probably be assisted by another arrangement of the same fibres which we have been describing; for some of these muscular bands are attached by their inner extremity to the anterior part of the left auriculo-tendinous ring, so as to form loops, the greater part of which lie more in front than behind the axis of the heart, and may have a tendency, when in a state of contraction, to draw the apex forwards and upwards. Now when we remember that by this elevation of the apex forwards, the heart, before placed obliquely, now becomes more horizontal, and consequently more approximated to the walls of the chest—the more particularly as the transverse diameter of the chest diminishes rapidly as we proceed from below upwards,—we believe that we have here sufficient to account for this impulse against the chest. As the proximity of the apex of the heart to the chest is affected by the position of the body, as we have already pointed out, this circumstance ought to be attended to in judging of the strength of the impulse of the heart.

No. XII.

ON THE MEASUREMENTS OF THE HEART.

(EXTRACTED FROM THE LONDON AND EDINBURGH MONTHLY JOURNAL FOR MAY 1843.)

THE average weight of the heart in the cases in which we have weighed that organ is greater than we expected, after being acquainted with the results obtained by Dr. Clendinning,[1] and others which we have elsewhere collected on this point.[2] I had intended to have taken the measurements of the different parts of a large number of hearts, both in the healthy and diseased states; but the number of healthy hearts measured, when I resigned my official situation in the Infirmary, amounted only to thirty-eight. In taking the measurements of the length and breadth of the different cavities of the heart, it is impossible, as all those who have made the attempt must be aware, to be quite accurate to a line; for the edges or boundaries of the surfaces to be measured are not sufficiently sharp and defined to admit of this. The thickness of the walls of the ventricles can be more accurately measured by employing a pair of compasses, or some such instrument, to ascertain the exact thickness of the walls, independent of any inequalities

[1] Medico-Chirurgical Transactions, vol. xxi. 1838.

[2] Article *Heart*, in Todd's Cyclopædia of Anatomy and Physiology, vol. ii.

arising from the manner in which they may be divided. The auriculo-ventricular and arterial orifices were first cut through, then spread out on a table, and the distance between the cut ends of the tendinous rings which surround these orifices was taken as the amount of their circumference. In measuring 16 adult male hearts between 9 and $13\frac{1}{2}$ ounces, and 9 adult female hearts between 7 and 12 ounces, the following results were obtained:—

	MALES.			FEMALES.		
	Minimum inches.	Maximum inches.	Medium inches.	Minimum inches.	Maximum inches.	Medium inches.
Length of anterior surface of left ventricle,	$4\frac{3}{10}$	$5\frac{2}{10}$	$4\frac{6}{10}$	$3\frac{8}{10}$	$4\frac{9}{10}$	$4\frac{3}{10}$
... ... right ditto,	$4\frac{1}{10}$	5	$4\frac{5}{10}$	$3\frac{8}{10}$	5	$4\frac{3}{10}$
... posterior surface of left ditto,	$3\frac{1}{10}$	$4\frac{4}{10}$	$3\frac{6}{10}$	$2\frac{9}{10}$	$3\frac{6}{10}$	$3\frac{3}{10}$
... ... right ditto,	$3\frac{1}{10}$	$4\frac{3}{10}$	$3\frac{5}{10}$	$2\frac{9}{10}$	$3\frac{6}{10}$	$3\frac{2}{10}$
Breadth of anterior surface of left ventricle,	$1\frac{4}{10}$	$2\frac{4}{10}$	$2\frac{1}{20}$	$1\frac{6}{10}$	2	$1\frac{8}{10}$
... ... right ditto,	$2\frac{5}{10}$	$4\frac{1}{10}$	$3\frac{5}{10}$	$2\frac{4}{10}$	$3\frac{4}{10}$	$2\frac{9}{10}$
... posterior surface of left ditto,	$2\frac{5}{10}$	$3\frac{7}{10}$	$3\frac{1}{20}$	$2\frac{3}{10}$	$2\frac{9}{10}$	$2\frac{6}{10}$
... ... right ditto,	$1\frac{6}{10}$	$2\frac{8}{10}$	$2\frac{1}{10}$	$1\frac{6}{10}$	2	$1\frac{9}{10}$
Thickest part of walls of left ventricle,	$\frac{9}{24}$	$\frac{16}{24}$	$\frac{12}{24}$	$\frac{8}{24}$	$\frac{4}{24}$	$\frac{10}{24}$
Thickness of middle part of ditto, . .	$\frac{8}{24}$	$\frac{12}{24}$	$\frac{9}{24}$	$\frac{7}{24}$	$\frac{12}{24}$	$\frac{8}{24}$
... one inch above apex of ditto,	$\frac{6}{24}$	$\frac{11}{24}$	$\frac{8}{24}$	$\frac{6}{24}$	$\frac{8}{24}$	$\frac{7}{24}$
... of apex of ditto, ...	$\frac{2}{24}$	$\frac{6}{24}$	$\frac{7}{48}$	$\frac{2}{24}$	$\frac{4}{24}$	$\frac{5}{48}$
Thickest part of right ventricle, . .	$\frac{3}{24}$	$\frac{5}{24}$	$\frac{4}{24}$	$\frac{3}{24}$	$\frac{5}{24}$	$\frac{7}{48}$
... of septum of heart, . .	$\frac{8}{24}$	$\frac{15}{24}$	$\frac{12}{24}$	$\frac{6}{24}$	$\frac{11}{24}$	$\frac{9}{24}$
Circumference of right auriculo-ventricular orifice, ...	$4\frac{9}{10}$	$5\frac{6}{10}$	$5\frac{3}{10}$	$4\frac{4}{10}$	$5\frac{2}{10}$	$4\frac{9}{10}$
... of left ditto, ...	$4\frac{1}{10}$	$5\frac{3}{10}$	$4\frac{6}{10}$	$3\frac{9}{10}$	$4\frac{4}{10}$	$4\frac{2}{10}$
... of orifice of pulmonary artery, ...	3	$4\frac{2}{10}$	$3\frac{7}{10}$	$3\frac{1}{10}$	$3\frac{7}{10}$	$3\frac{5}{10}$
... of orifice of aorta, . .	$2\frac{6}{10}$	$3\frac{8}{10}$	$3\frac{2}{10}$	$2\frac{2}{10}$	$3\frac{4}{10}$	3

In the above measurements the average breadth of the anterior surface of the right over the left ventricle is $1\frac{4}{10}$ inch nearly, and in the female $1\frac{1}{10}$ inch; while on the posterior surface the breadth of the left ventricle exceeds that of the right by $\frac{9}{10}$ of an inch in the male, and $\frac{7}{10}$ in the female. The anterior surface of each ventricle is about an inch longer than its posterior surface. The average length of the corresponding surfaces of the ventricles is

very nearly the same, but the left is slightly longer on the posterior surface. Both in the male and female the left ventricle is about three times thicker than the right. Both in the male and female the circumference of the right auriculo-ventricular orifice is $\frac{7}{10}$ of an inch longer than the left; and the pulmonary orifice is $\frac{5}{10}$ of an inch longer than that of the aorta.

No. XIII.

TABLES OF THE WEIGHTS OF SOME OF THE MOST IMPORTANT ORGANS OF THE BODY AT DIFFERENT PERIODS OF LIFE.

(EXTRACTED FROM THE LONDON AND EDINBURGH MONTHLY JOURNAL OF MEDICAL SCIENCE FOR APRIL 1843.)

The materials for the following Tables were collected while I was attached to the Edinburgh Royal Infirmary. As my present position does not afford me many opportunities of adding to these, my store of facts is much less complete than I had intended. No one can be more perfectly satisfied than myself, that though, at first sight, the data I have amassed appear sufficiently ample to enable us to draw satisfactory conclusions regarding the average weight of some of the most important organs of the body at different periods of life, yet they are, when more narrowly examined, much too scanty for the purposes intended. Comparatively few children are received into the Infirmary, and the greatest imperfections in the tables I have constructed are consequently to be found at the earlier periods of life. As the number of facts which I have collected regarding the weight of several internal organs of the body is, however, so considerable, I think it right to make them public in such a form that they may

be incorporated with other facts of the same kind; and it is with this view that I have given all the data in detail. I had at first intended to give only the tables founded upon and appended to the detailed data or individual facts; but after reflecting on the matter, I became satisfied that the latter were more valuable than the former. The detailed facts may be incorporated with other identical facts collected by other individuals, and the basis for sound and correct deductions be thus enlarged, and rendered more trustworthy; while the tables founded upon the detailed data cannot, in general, be incorporated with other tables drawn up by other labourers in the same field, and are often totally unserviceable when we wish to throw the facts into new arrangements. It not unfrequently happens, that different statistical collectors are more anxious to contrast the conclusions which they have deduced from the limited data procured by themselves with those drawn from equally circumscribed sources by others, than to admit that more accurate deductions might be drawn from an accumulation of *all* the identical facts collected by competent observers. Statisticians should look upon each other more in the light of allies than as antagonists. The accumulation of individual facts on the subject with which we have at present to do, is chiefly valuable in pointing out the great variety in the weight of the same organs in a state of health in different individuals of the same age—thus enforcing upon us the sources of fallacy to which we are liable in drawing our averages from a small number of cases, and impressing upon us the insufficiency of any comparison between the weight of a diseased organ in any individual case and the *average weight* of a healthy organ, at the same period of life, in enabling us to form any correct estimate of the change in weight which it has undergone in consequence of morbid action, or other causes. A knowledge of the *average* in this, as in other cases of medical statistics,

should only be considered as the preparatory step for the more successful investigation of the circumstances upon which the numerous deviations from the average depend. Among the adult male brains which I have weighed, I have found as great a difference as $28\frac{1}{2}$ oz. between two brains—the one being about $12\frac{1}{4}$ oz. above the average weight, and the other about $16\frac{1}{4}$ oz. below it. It would be waste of time to dwell upon the errors which might be committed in applying average weights in reasoning upon the effects of any particular changes upon the brain in these two individual cases.

In weighing the entire bodies in the subjoined tables, I took care to exclude all those in which any considerable inflammatory or dropsical effusion was found, and where there was much obesity. All the individual organs which did not appear quite healthy were also invariably rejected from the list of "weight of healthy organs." In weighing the encephalon, and the different parts of it found in the tables, the following was the procedure adopted:—The skull-cap being removed, and the dura mater cut through, the hemispheres of the cerebrum were sliced cautiously down, the lateral ventricles were opened, and the serum in their interior withdrawn by a pipette, and measured in a graduated glass vessel. The medulla spinalis was cut through on a level with the margin of the foramen magnum, and the whole encephalon was then weighed. The cura cerebri were then cut through as they emerge from the upper edge of the pons Varolii; and the cerebellum, medulla oblongata, and pons Varolii, were thus weighed together. To weigh the cerebellum separately, the pons Varolii and medulla oblongata were then detached by cutting through the cura cerebelli as they pass into the lateral lobes of the cerebellum. Before the heart was weighed all the cavities had been opened, and emptied of their fluid and coagulated blood. In removing the heart the

large veins were cut across where they enter the auricles; but about an inch of each of the two large arteries leaving the heart was left attached to it, and weighed along with it. All the hearts presenting any considerable quantity of fat on their outer surface were not weighed. In all cases the weight used was avoirdupois.

[I have in this volume given only the tables founded upon those containing the weight of each separate organ. All the tables containing the materials from which the general tables were constructed, are printed in the Monthly Journal of Medical Science for April 1843.]

TABLE I.

WEIGHT USED AVOIRDUPOIS.

Exhibiting the Heaviest, Lightest, and the Average Weight of Encephalon, Cerebellum, and Cerebellum with Pons Varolii and Medulla Oblongata, at different ages, in 253 Brains. Though individual female brains are not unfrequently found to be heavier than individual male brains, yet as the average male brain is several ounces heavier than the average female brain, it is necessary that these be ranged in separate tables; the more especially when the number of brains at different ages, weighed in the two cases, do not correspond.

	Age.	Number weighed.	Heaviest.						Lightest.						Average.					
			Encephalon.		Cerebellum.		Cerebellum with pons and medulla.		Encephalon.		Cerebellum.		Cerebellum with pons and medulla.		Encephalon.		Cerebellum.		Cerebellum with pons and medulla.	
	Years.		oz.	dr.	oz.	dr.	oz.	dr.	oz.	dr.	oz.	dr.	oz.	dr.	oz.	dr.	oz.	dr.	oz.	dr.
MALES.	1—4	5	45	4	4	10	5	6	27	8	2	8	3	0	39	$4\frac{2}{5}$	3	$13\frac{1}{5}$	4	$6\frac{2}{5}$
	5—7	3	47	$10\frac{1}{2}$	5	1	6	0	40	12	4	0	4	9	43	10	4	7	5	6
	7—10	6	52	14	5	0	5	11	40	12	4	0	4	10	46	$2\frac{4}{6}$	4	$10\frac{2}{5}$	5	$10\frac{6}{7}$
	10—13	3	51	2	5	2	6	2	43	8	4	9	5	6	48	$7\frac{1}{3}$	4	14	5	12
	13—16	5	50	2	5	8	6	8	43	10	0	0	5	10	47	$8\frac{2}{5}$		...	6	$1\frac{1}{2}$
	16—20	6	56	0	6	1	7	2	48	0	4	8	5	8	52	$10\frac{2}{3}$	5	$4\frac{1}{2}$	6	$6\frac{2}{10}$
	20—30	25	58	0	6	0	7	0	45	8	4	12	5	4	50	$9\frac{1}{2}$	5	$3\frac{1}{2}$	6	2
	30—40	23	62	8	5	14	8	8	40	10	4	6	5	1	51	$15\frac{1}{2}$	5	$3\frac{1}{2}$	6	$4\frac{1}{2}$
	40—50	34	53	8	6	4	7	10	34	0	4	8	5	6	48	$13\frac{1}{2}$	5	$3\frac{2}{6}$	6	$4\frac{6}{11}$
	50—60	29	59	0	7	0	8	4	39	0	4	8	5	4	50	2	5	$5\frac{8}{18}$	6	$2\frac{2}{28}$
	60—70	8	60	4	6	3	7	4	40	0	4	2	5	2	50	$6\frac{6}{8}$	5	0	6	2
	70 and upwards,	7	54	10	5	8	6	8	43	8	4	8	5	4	48	$4\frac{2}{7}$	4	14	5	$14\frac{4}{7}$
	Total male brains weighed,	154																		
FEMALES.	2—4	6	42	0	4	0	4	10	32	0	3	5	3	15	37	9	3	$9\frac{8}{10}$	4	5
	5—7	3	41	8	4	0	4	8	36	0	3	5	4	0	39	$9\frac{1}{3}$	3	11	4	$8\frac{2}{3}$
	7—8	3	43	14	4	10	5	9	40	8	4	4	5	0	42	$7\frac{1}{3}$	4	$7\frac{4}{6}$	5	5
	10—13	1	43	8	5	2	6	2	...		...		...		...		...		...	
	13—16	1	41	0	4	8	5	8	...		...		...		...		...		...	
	16—20	8	49	12	5	8	6	4	41	8	4	12	5	6	44	$11\frac{1}{2}$	4	$14\frac{1}{6}$	5	$11\frac{12}{14}$
	20—30	18	50	0	5	2	6	2	39	2	4	0	4	12	45	$2\frac{2}{9}$	4	$11\frac{1}{3}$	5	$9\frac{1}{3}$
	30—40	23	51	0	5	8	6	8	39	14	4	2	5	0	44	$1\frac{1}{2}$	4	$13\frac{1}{5}$	5	11
	40—50	18	50	6	6	0	7	0	36	12	3	12	4	4	44	$10\frac{4}{9}$	4	14	5	$14\frac{4}{7}$
	50—60	5	48	6	4	12	5	15	43	4	4	4	5	2	45	$4\frac{4}{5}$	4	$7\frac{1}{2}$	5	$8\frac{2}{5}$
	60—70	11	46	10	5	2	6	0	36	2	4	2	5	0	42	$14\frac{6}{7}$	4	$10\frac{2}{11}$	5	9
	70 and upwards,	2	46	0	5	1	6	0	31	1	3	10	4	5	38	$8\frac{1}{2}$	4	$5\frac{1}{2}$	5	$2\frac{1}{2}$
	Total female brains weighed,	99																		

TABLE II.

Relative Weight of Cerebellum, and of Cerebellum with Pons Varolii, and Medulla Oblongata to the Encephalon, at different ages, in 172 bodies.[1]

	MALES.				FEMALES.			
Ages.	Cerebellum.	Number weighed.	Cerebellum with pons varolii and medulla.	Number weighed.	Cerebellum.	Number weighed.	Cerebellum with pons and medulla oblongata.	Number weighed.
1—5 years,	1 to $10\frac{2}{5}$	5	1 to $8\frac{15}{16}$	5	1 to $9\frac{9}{10}$	4	1 to $8\frac{10}{17}$	5
5—7 ,,	1 $9\frac{9}{10}$	3	1 $8\frac{1}{6}$	3	1 $10\frac{7}{10}$	2	1 $8\frac{3}{5}$	3
7—10 ,,	1 $9\frac{5}{12}$	5	1 $8\frac{1}{23}$	5	1 $9\frac{1}{2}$	3	1 8	3
10—13 ,,	1 $9\frac{20}{23}$	3	1 $8\frac{5}{12}$	3	...	...	...	...
13—15 ,,	1 $9\frac{1}{11}$	1	1 $7\frac{13}{15}$	4	...	...	...	...
16—20 ,,	1 $9\frac{15}{17}$	4	1 $8\frac{7}{18}$	4	1 $9\frac{1}{19}$	5	1 $7\frac{3}{4}$	5
20—30 ,,	1 $9\frac{11}{18}$	13	1 $8\frac{1}{6}$	13	1 $9\frac{9}{25}$	12	1 8	12
30—40 ,,	1 $9\frac{15}{23}$	11	1 $8\frac{7}{45}$	11	1 $9\frac{6}{11}$	15	1 $8\frac{1}{17}$	15
40—50 ,,	1 $9\frac{8}{18}$	23	1 8	23	1 $9\frac{1}{9}$	9	1 $7\frac{20}{27}$	9
50—60 ,,	1 $9\frac{11}{16}$	17	1 $8\frac{1}{8}$	17	1 10	4	1 $8\frac{2}{7}$	4
60—70 ,,	1 $10\frac{4}{13}$	5	1 $8\frac{1}{4}$	8	1 $9\frac{8}{30}$	11	1 $7\frac{24}{32}$	11
70 and upwards,	1 $9\frac{3}{8}$	5	1 $8\frac{1}{6}$	7	1 $8\frac{12}{13}$	2	1 $8\frac{2}{25}$	2
$2\frac{1}{2}$ years,	...	...	...	...	1 $10\frac{1}{22}$	4	1 $8\frac{1}{2}$	4[2]
4 months,	1 11	1	1 $9\frac{1}{6}$	1	...	...	...	...
1 year,	1 $9\frac{15}{16}$	1	1 $8\frac{15}{18}$	1	...	...	...	...

An examination of Table I. does not afford any support to the supposition of some, that the cerebellum attains its maximum weight at seven years of age, and the cerebrum its maximum weight nearly at the same period, or only a little later. There appears to be little doubt, however, from all the facts which have been collected on this subject, that the brain approaches its maximum weight sooner

[1] In ascertaining the relative weight of the cerebellum to the encephalon, those encephala only were selected in which the cerebella were also weighed. The same plan was also followed in ascertaining the relative weight of the cerebellum with pons and medulla oblongata to the encephalon.

[2] Three of the youngest cases included in the above Table given separately.

than the other organs of the body, and to judge from a few measurements we have made of the length of the corpus callosum, the depth of the grey matter, the length, breadth, and depth of the corpus striatum and thalamus, we would be inclined to conclude that the relative size of these parts is the same in the young person as in the adult. We believe that there can be little doubt that the relative size of the brain to the other organs, and to the entire body, is much greater in the child than in the adult. In Table III. will be found the results we have obtained on this point. In Table II. we find less difference between the relative weight of the encephalon and cerebellum, at different periods of life, than we had been led to expect from some statements which have been made upon this question. The data we have collected do not entitle us to speak positively, but as the other statements to which I refer seem principally to rest upon the vague and uncertain measurements of the eye, we may reasonably request to be allowed to suspend our opinion of their accuracy, until we have a sufficient amount of materials brought before us to justify us in giving a decided judgment. In looking over the column of the average weights of the encephalon, at different ages, in Table I., we cannot fail to experience some surprise at the difference between the average weight of that organ in the male, between 16 and 20 years of age, and between 40 and 50, but we cannot for a moment have any hesitation in deciding that this must arise from sources of fallacy incident to insufficient data. In the group between 40 and 50 years of age some brains much below the average weight are found, and there can be no doubt that it is to this accidental circumstance that we must attribute the diminution in the average weight of the brain in that group. Among the females we find a decided diminution in the average weight of the brain above 60 years of age, while among the males this is not apparent until a later

period. We certainly did expect also to find a similar diminution in the average weight of the male brain above 60 years of age, for we are perfectly satisfied, as the tables containing the individual facts will show, that we more frequently meet with a greater quantity of serum under the arachnoid and in the lateral ventricles in old people, than in those in the prime of life. We are also satisfied, from an examination of the notes we have taken at the time the brains were examined, that a certain degree of atrophy of the convolutions of the brain over the anterior lobes, marked by the greater width of the sulci, was more common in old than in young persons. We have, however, frequently remarked these appearances in the brains of people in the prime of life, who had been for some time addicted to excessive indulgence in ardent spirits.

TABLE III.

Relative Weight of Encephalon, Cerebrum, Cerebellum, Cerebellum with Pons Varolii and Medulla Oblongata, Heart, and Liver, to the entire Body, in 92 Bodies. In this and in all other similar tables, I have selected those cases only in which all the organs whose relative weight is given were weighed in the same individual.

	Ages.	Encephalon to Body.	Number weighed.	Cerebrum to Body.	Number weighed.	Cerebellum to Body.	Number weighed.	Cerebellum with pons varolii and medulla.	Number weighed.	Heart to Body.	Number weighed.	Liver to Body.	Number weighed.
MALES.	1—5 years,	1 to $8\frac{1}{2}$	4	1 to $9\frac{6}{10}$	4	1 to $88\frac{1}{3}$	4	1 to $76\frac{1}{3}$	4	1 $176\frac{3}{4}$	4	1 $21\frac{2}{9}$	5
	at 5 years,	1 $9\frac{15}{19}$	2	1 $10\frac{4}{27}$	2	1 $97\frac{4}{9}$	2	1 $81\frac{3}{33}$	2	1 $130\frac{8}{15}$	2	1 $23\frac{12}{16}$	2
	at 7 years,	1 $10\frac{24}{71}$	2	1 $11\frac{12}{17}$	2	1 $107\frac{4}{7}$	2	1 $93\frac{21}{23}$	2	1 $175\frac{1}{3}$	1	1 21	1
	13—15 years,	1 $15\frac{10}{14}$	3	1 $21\frac{18}{21}$	3	1 $142\frac{12}{22}$	1	1 $146\frac{3}{7}$	3	1 $176\frac{12}{16}$	2	1 $25\frac{24}{28}$	2
	20—30 „	1 $35\frac{42}{44}$	11	1 $40\frac{27}{38}$	11	1 $352\frac{12}{20}$	10	1 $293\frac{12}{27}$	11	1 $173\frac{5}{7}$	13	1 $29\frac{3}{5}$	6
	30—40 „	1 $37\frac{6}{11}$	6	1 $41\frac{2}{25}$	5	1 $342\frac{11}{21}$	5	1 $306\frac{15}{19}$	6	1 $165\frac{25}{27}$	6	1 $35\frac{1}{2}$	7
	40—50 „	1 38	14	1 $42\frac{19}{41}$	12	1 $348\frac{1}{27}$	12	1 $295\frac{8}{11}$	12	1 $169\frac{8}{9}$	11	1 $35\frac{4}{6}$	14
	50—60 „	1 $36\frac{5}{7}$	11	1 $42\frac{1}{3}$	10	1 $370\frac{1}{3}$	8	1 $318\frac{5}{7}$	10	1 $165\frac{4}{11}$	15	1 $35\frac{1}{4}$	4
	60—70[1] „	1 $39\frac{1}{5}$	4	1 $44\frac{1}{6}$	4	1 $427\frac{1}{3}$	4	1 $348\frac{1}{2}$	4	1 137	3	1 $43\frac{3}{9}$	2
FEMALES.	2—4 „	1 $8\frac{5}{9}$	4	1 $9\frac{7}{10}$	4	1 $84\frac{1}{14}$	4	1 $71\frac{11}{12}$	4	1 $151\frac{1}{10}$	6	1 20	5
	5—7 „	...	...	...	...	...	...	...	...	...	...	...	...
	7—10 „	1 $13\frac{1}{23}$	3	1 $15\frac{1}{13}$	3	1 125	3	1 $105\frac{10}{25}$	3	...	...	1 $22\frac{1}{4}$	1
	13—15 „	1 22	...	...	...	...	...	...	...	...	...	...	...
	16—20 „	1 $30\frac{1}{2}$	3	1 $31\frac{9}{12}$	3	1 $283\frac{1}{3}$	3	1 $181\frac{1}{3}$	3	1 181	4	1 $30\frac{5}{12}$	4
	20—30 „	1 $33\frac{4}{23}$	4	1 $37\frac{3}{4}$	4	1 $327\frac{51}{15}$	4	1 $275\frac{1}{2}$	4	1 $183\frac{13}{14}$	7	1 $33\frac{2}{7}$	5
	30—40 „	1 $34\frac{6}{9}$	8	1 $39\frac{3}{7}$	6	1 $316\frac{2}{5}$	5	1 $285\frac{3}{4}$	6	1 $173\frac{3}{4}$	5	...	...
	40 50 „	1 35	5	1 $41\frac{2}{5}$	4	1 $324\frac{6}{13}$	4	1 $277\frac{1}{2}$	4	1 $174\frac{1}{6}$	6	1 $42\frac{1}{6}$	4
	50—60 „	1 $38\frac{1}{53}$	2	1 $41\frac{1}{2}$	2	1 $370\frac{1}{11}$	2	1 $307\frac{55}{83}$	2	...	...	...	...
	60 and upwards,	1 $38\frac{1}{5}$	6	1 $43\frac{1}{5}$	6	1 $346\frac{1}{3}$	6	1 $288\frac{7}{10}$	6	1 $180\frac{11}{27}$	4	1 $25\frac{1}{4}$	2

[1] One of these was above 70 years of age.

TABLE IV.

Average weight of the Encephalon, &c., between 25 and 55 years of age, in the two sexes, and the average difference between them.

Males, 53 brains weighed.—Females, 34 brains weighed.

	Male.	Female.	Difference in favour of the Male.
	lb. oz. dr.	lb. oz. dr.	oz. dr.
Average weight of Encephalon, ...	0 50 $3\frac{1}{2}$, or 3 2 $3\frac{1}{2}$	0 44 $8\frac{1}{2}$, or 2 12 $8\frac{1}{2}$	5 11
Do. Cerebrum, . .	0 43 $15\frac{3}{4}$	0 38 12	5 $3\frac{3}{4}$
Do. Cerebellum, . .	0 5 4	0 4 $12\frac{1}{4}$	0 $7\frac{3}{4}$
Do. Cerebellum with pons and medulla oblongata, . .	0 6 $3\frac{3}{4}$	0 5 $12\frac{1}{3}$	0 $7\frac{1}{2}$ nearly

TABLE V.

Relative weight of Cerebellum, and of Cerebellum with Pons Varolii and Medulla Oblongata, to the Encephalon, between 25 and 55 years of age, in the two sexes.

53 male and 34 female brains weighed.

	Male.	Female.
Relative weight of Cerebellum to Encephalon, as	1 to $9\frac{4}{7}$	1 to $9\frac{1}{4}$
Do. do. Cerebellum with Pons and Medulla, to Encephalon, ... as	1 $8\frac{8}{13}$	1 $7\frac{9}{10}$

From this Table it would appear that, in the female, the average Cerebellum is—relative to the Encephalon—a little heavier than in the male.

TABLE VI.

Relative weight of the Encephalon, the Heart, and Liver, to the entire Body in the two sexes, between 25 and 55 years of age.

	Encephalon.	Number weighed.	Heart.	Number weighed.	Liver.	Number weighed.
Male, as	1 to $37\frac{1}{2}$	33	1 to $169\frac{1}{2}$	37	1 to $35\frac{1}{3}$	31
Female,	1 35	15	1 176	12	1 39	7

As far as this Table enables us to judge, it would appear that though the average male brain is absolutely heavier than that of the female, yet that the average female brain, relative to the weight of the whole body, is somewhat heavier than the average male brain.

TABLE VII.

In 9 Males, between 27 and 50 years of age, who died either immediately, or within a few hours after accidents and other external causes of sudden death, and who had been previously in good health, the following results were obtained :—

Average weight of body, (9 weighed.)	Average of encephalon, (6 weighed.)	Average of cerebellum, (4 weighed.)	Average of cerebellum with pons and medulla oblongata, (5 weighed.)	Average of heart, (9 weighed.)
st. lb. oz. 9 8 $3\frac{1}{2}$, or 0 134 $3\frac{1}{2}$	lb. oz. dr. 0 52 $4\frac{4}{6}$, or 3 4 $4\frac{4}{6}$	oz. dr. 5 $7\frac{1}{5}$	oz. dr. 6 6 or, taking the average of the four cases only in which the cerebellum was taken, 6 oz. $7\frac{7}{8}$ dr.	oz. dr. 12 6

Proportion of encephalon to body,	(6 weighed)	as 1 to	$40\frac{4}{5}$
Do. heart, do.	(9 weighed)	1	$173\frac{1}{3}$
Do. cerebellum to encephalon,	(4 weighed)	1	$9\frac{2}{5}$
Do. cerebellum with pons and medulla, do.	(5 weighed)	1	$8\frac{6}{10}$

Though the data from which the above Table is constructed are very limited, yet we may be allowed to remark, that the greater relative weight of the encephalon to the body, in those emaciated by disease than in those cut off while in possession of health and muscular vigour, which it indicates, is what we would expect from other considerations. There is little difference in the relative weight of the cerebellum to the encephalon in the two classes of cases.

TABLE VIII.

Average weight of Heart, Liver, and Kidneys, between 25 and 55 years, in the two sexes.

Heart.		Liver.		Kidneys.	Right.	Left.	Difference between the two kidneys in the same sex.
	oz. dr.		oz. dr.		oz. dr.	oz. dr.	oz. dr.
M. (89 weighed)	11 1	M. (60 weighed)	52 $12\frac{1}{2}$	M. (65 weighed)	5 7	5 $11\frac{1}{2}$	0 $4\frac{1}{2}$
F. (53 do.)	9 $0\frac{1}{2}$	F. (25 do.)	45 $3\frac{1}{2}$	F. (28 do.)	4 13	5 2	0 5
Total, 142 Differ.	2 $0\frac{1}{2}$	Total, 85 Differ.	7 9	Total, 93 Differ.	0 10	0 $9\frac{1}{2}$	

Though the right kidney is sometimes equal in weight, and occasionally heavier than the left, yet the average left kidney is heavier than the right. In the above Table the average left kidney is $4\frac{1}{2}$ drachms heavier than the right in the male, and 5 drachms in the female.

The weight of the lungs is so much modified by the quantity of blood which they may contain, that I have not attempted to ascertain their relative weight to the other organs. The right lung is almost invariably a few ounces heavier than the left, and it appeared to me that when the left was heavier, this was dependent upon the greater quantity of blood contained in it.

[Dr. Peacock, my successor in the office of Pathologist to the Edinburgh Royal Infirmary, who published additional Tables of the weights of some of the organs of the body, (Monthly Journal of Medical Science for August and September 1846,) containing the weights of 105 encephala in addition to the 253 from which I constructed the preceding Tables, has incorporated his data with mine, and has thus enlarged the basis for procuring more trustworthy deductions. The average weight of the encephalon in 131 males between 25 and 55 years of age, 41 of these were weighed by Dr. Peacock, and 90 by me—was found to be 50 oz. $3\frac{54}{131}$ dr. : while the average weight of this organ in 74 females within the same periods of life, 21 of which were weighed by Dr. Peacock, and 53 by me—was found to be 44 oz. $14\frac{28}{74}$ dr., giving a difference of 5 oz. 4.95 dr., in favour of the male encephalon. Of these 131 male encephala, the heaviest was 62 oz. 12 dr., and the lightest 34 oz. : and of the 74 female encephala, the heaviest was 54 oz., and the lightest 36 oz. 12 dr. The average weight of the cerebrum in 95 males between 25 and 55 years of age, was found to be 44 oz. 3.4 dr., and the average weight of this organ in 58 females, within the same periods of life, was found to be 39 oz. 3.3 dr., giving a difference of 5 oz. 0.1 dr. in favour of the male cerebrum. Dr. Peacock, on adding together the facts collected by me and by himself, concludes from them, that the proportion which the cerebellum with the pons varolii and medulla oblongata bears to the whole encephalon in the adult is 1 to 7.8, and is nearly the same in the two sexes, being as 1 to 8.057 in the male, and 1 to 7.87 in the female. Dr. Sharpey and Mr. Quain, in their new edition of Dr. Quain's Anatomy, have thrown into a single Table the weights of the encephalon in both sexes above 21 years of age, collected by Dr. Sims, Dr. Clendinning, Tiedemann and

myself.[1] In this Table a very large proportion of the weights of the male encephalon ranges between 46 oz. and 53 oz., and that of the female encephalon between 41 oz. and 47 oz. I weighed the spinal cord of a male aged 50, and found it to be 1 oz. 4 dr.]

[1] Part II., p. 668. From these different sources the weights of 278 male encephala, and of 191 female encephala, are collected.

No. XIV.

SOME OBSERVATIONS ON THE STRUCTURE OF THE MESENTERIC GLANDS IN THE BALAENOPTERA ROSTRATA.

(FROM THE EDINBURGH MEDICAL AND SURGICAL JOURNAL, JANUARY, 1835.)

Dr. Knox having lately purchased a young whale, he very kindly allowed me a considerable portion of the mesentery, cut close to the spine, for the purpose of repeating the experiments of Mr. Abernethy upon the organs in the whale that correspond to the mesenteric glands, as related in the Philosophical Transactions of London for 1796. This animal was a female, about ten feet in length, belonging to the *Balaenoptera rostrata* of naturalists, and had been dead about a fortnight (February.)

An injection, composed of brown spirit varnish, white spirit varnish, and turpentine varnish, mixed in the usual proportions, and coloured with vermilion, was first thrown into the arteries, and the same materials coloured yellow into the veins. Both injections succeeded well, and very minute vessels were seen ramifying in all directions upon the surface of the intestines and mesenteric glands. The mesenteric glands appeared a little increased in size after the injection had been thrown in ; but on handling them, the materials injected could not be felt to be collected into masses in their interior, as described by Mr. Abernethy,

except in one case, and this was evidently dependent upon extravasation. The lacteals were next attempted to be injected. These were more easily found than could have been expected, as the mesentery was completely void of fat, and all the small arteries and veins were filled with injection. Several of these vessels were injected; but in all, from the softness of the glands, or some other cause, the mercury seemed to burst into the substance of the gland, converting it into a sac, and none of it appeared in the *vasa efferentia*. On opening one of these glands, I was at first almost convinced that the mercury had passed into a cavity in the interior of the gland; and I had almost persuaded myself of the existence of an indistinct lining membrane upon its walls; but upon more minute examination of the others, and particularly as none of the mercury could be detected in the *vasa efferentia*, and as no such cavity could be detected in those not injected, I became satisfied that it had ruptured the vessels, and passed into the substance of the gland. On cutting into the substance of the glands in which the arteries and veins were only injected, these vessels were seen ramifying through their texture, dividing and subdividing very minutely, and nowhere presented the appearance of opening into cavities or cells. It also appeared to Dr. Alison, who examined some of these glands under the microscope, that the whole of the injection was contained in the vessels. The glands themselves were of a uniform texture; and, with the exception of being of a darker colour, and of a greater size, resembled very closely in their appearance the mesenteric glands of the human body.

Those who are acquainted with Mr. Abernethy's experiments, will at once perceive that the results of the injections just described differ very materially from those obtained by that gentleman. Upon what this difference depends I cannot pretend to judge. Mr. Abernethy does not men-

tion the genus, species, or size of the whale from which he procured the mesentery; but it is evident, from some parts of his paper, that the mesenteric glands must have been much larger than those in the present case. The largest of these bodies in this case did not exceed a pigeon's egg; and when they appeared larger, this was found to depend upon two or more of these lying in close contact. It is not likely that these bodies can differ so very much in their structure in the different Cetacea as to account for results so dissimilar; and the disparity in size could only produce a difference in degree, not in character. Mr. Abernethy used a waxen injection, which is considerably coarser than that which we employed. It appeared to Mr. Abernethy that some of the arteries and veins opened upon the inner surface of large cavities in the interior of the glands, into which they poured their contents; while in the present case, though the injection had run more minutely than what we could expect a waxen injection to do, none of it seemed to pass out of the vessels. It also appeared to Mr. Abernethy, that a number of lacteals opened into the interior of these cavities, and probably mixed their contents with those of the arteries and veins. Whether there were any communications between the lacteals and veins, or whether the lacteals poured any of their contents into cavities or cells, these injections do not entitle us to form any thing like a satisfactory conclusion; for the extravasation of the mercury into the substance of the gland prevented all accurate observation, and besides, the whole of the minute veins being filled with injections, it would be prevented from appearing in them, admitting that such communications did exist. We saw enough, however, to render it very improbable that the lacteals in this case opened into such large cavities as those described by Mr. Abernethy. Dr. Knox himself injected some eighteen or twenty of the lacteals two days previous to my attempts,

immediately after the abdomen had been opened, before the mesentery had been detached, before the arteries and veins had been injected, and while the parts were still very fresh. The mercury passed freely through these vessels as far as the glands; but none of it appeared either in the veins or *vasa efferentia.* The experiment of Mr. Abernethy has been supposed by some to furnish a strong analogical argument in favour of the communication between the veins and lymphatic vessels in the human body, but it appears to me, that this would require to be confirmed by others before much weight can be attached to it.

No. XV.

SOME OBSERVATIONS ON PHLEBOLITES.

(FROM EDINBURGH MEDICAL AND SURGICAL JOURNAL, JAN. AND APRIL 1835.)

THE accompanying preparation[1] is a portion of one of the broad ligaments of the uterus, in the veins of which is observed a number of small rounded osseous-looking bodies, which have received the name of Phlebolites. They are described by Beclard in his Anatomie Générale under the chapter on Veins, and by Jules Cloquet in his Pathologie Chirurgicale; but I am not aware of their having been noticed, except incidentally, by any English author, though they must have occasionally been seen by those much engaged in dissections. Their presence does not appear to be attended by any bad effects, and they consequently possess little practical importance; but the nature and cause of their formation may, ere long, become an object of considerable interest in physiology, as serving to strengthen or confirm some of the doctrines concerning the living fluids of the body. They cannot be considered as of rare occurrence. I have now seen them in five instances, and four of these have been within the last twelve months. In three of these they were found in the uterine veins; in one (a male) in the vesical veins; and in the present case, both in the vesical and uterine. They varied in size from that

[1] This paper had been read at a Meeting of the Edinburgh Medico-Chirurgical Society.

of a millet-seed to that of a large pea. They varied in number from two to more than a dozen. In all the cases in which I have examined them, they seemed to be placed loose in dilatations of the veins, allowing sufficient space for the blood to pass between them and the coats of the vein. Some of these veins were of very small size in the undilated portions, and required a careful examination to be convinced that they were placed in veins; while others were of sufficient size to render this apparent almost at the first glance. The subjects in which I found them were all advanced beyond the middle period of life.

Beclard and Cloquet describe them as being most frequently inclosed in layers of fibrinous matter or coagulated blood; but in all, except in the present case, I have found them without any covering whatever, and in nearly all of them of a bony hardness throughout.

In the preparation before you a very large one may be observed in a clot of blood. Several smaller ones were also observed enveloped in coagulated blood. The spermatic veins in this case, in the branches of which these were deposited, were much enlarged.

According to Cloquet and Beclard, they have been also found in the hæmorrhoidal veins, in the spermatic veins of the male, in the saphenæ veins, and twice in the *vena cava inferior*. In one of the cases described and delineated by Cloquet, he states, that the smaller ones were formed of a little soft clot of blood; that those of a larger size contained in the centre a nucleus of white fibrinous matter; that the largest had in their centre an osseous-looking nucleus, upon which the white fibrinous matter was arranged in concentric layers, the internal very hard, the others becoming gradually softer as they approached the surface. He also gives a drawing of a round fibrinous mass taken from the *vena cava inferior*, containing an ossific-looking matter in the centre, from which a number of rays of the

same substance passed through the fibrinous matter towards the circumference. Another specimen similar to this was seen by Cloquet in Sœmmering's museum, and was said to have been taken from the *vena cava inferior* of a child.

I am not aware that the chemical composition of these bodies has been accurately ascertained. Cloquet speaks of them as composed of phosphate of lime, but he does not state that they were analyzed. Mr. Kemp had the kindness to analyze two of them taken from different subjects. He has not yet been able to obtain accurate results; but he states, that he is certain that both were principally composed of phosphate of lime, of carbonate of lime, and of animal matter, and that it appeared to him that these earthy salts and animal matter were nearly in the proportions that exist in the bones. He has promised to make a more accurate analysis; and we will then see how far they differ from, or agree with, the osseous texture in their chemical composition. It may also be stated, that the animal matter seemed composed in a great part of gelatin; but this would require farther researches.

It becomes an interesting subject of inquiry, whether these bodies are formed by a disposition of the earthy salts contained in the blood, mixed with some of the animal matter, or whether this depends upon some vital action in the fluids, corresponding to the formation of the osseous texture. Neither Beclard nor Cloquet give any opinion upon the cause of their formation. The fact of their being only found in the depending veins, where the flow of blood is retarded, and the improbability of the formation of any osseous texture in the interior of these vessels, strongly favour the notion of their being merely depositions from the blood, as urinary calculi are from the urine; while the nature of their formation, the gradual conversion of the fibrinous layers into osseous, the radiation of the ossific

matter through the fibrinous clots, and their chemical composition (if their similarity to bone is confirmed,) must strongly incline us to ascribe their origin to something more than the ordinary physical or chemical laws. Additional observations, by furnishing more extensive data, may enable us to come to more decisive conclusions on this interesting question.

Since the preceding remarks on Phlebolites were written, I found, during the last summer, (1834,) several of these bodies in the vesical and uterine veins, which presented some unusual appearances. The largest was at least of the size of a garden-pea; the others were considerably smaller. The largest, and also several of the smaller, consisted of two distinct portions. The smaller portion, in the largest of these bodies, occupied one of the extremities, was smooth externally, of a bluish colour, and was nearly of the consistence of cartilage; the larger portion was of a bony hardness throughout. When dried, this smaller portion shrunk considerably, and resembled in appearance a piece of dried cartilage.

I find, upon more extensive inquiry, as I had previously expected, that many of those much engaged in dissection have observed these bodies more than once. I have found them in five out of the last sixty subjects in the dissection of which I have been concerned. My friend, Mr. Fergusson, informs me that he once found them in great quantity in the mesenteric veins.

Tiedemann gives the following analysis of them by Gmelin :—

Animal matter, . .	27.5
Phosphate of lime, . .	53.5
Carbonate of lime, . .	15.5
Magnesia and loss, . .	3.5
	100.0[1]

[1] Tiedemann's Zeitschrift für Physiologie, Vierter Band, Erstes Heft. 1831.

It is lately stated by Dr. Lee that Dr. Prout has also analyzed them, and has obtained similar results.[1] Though the different changes through which these bodies pass in their formation have now been pretty accurately ascertained, yet the nature of the causes which produce these changes is exceedingly obscure. Tiedemann, Otto,[2] Lobstein,[3] Cloquet, and Dr. Carsewell,[4] all agree that they are formed in the interior of the veins; that they at first consist of a coagulum of blood; in the interior of this clot the fibrin becomes pale and concrete, then assumes an osseous appearance, and this goes on little by little, and layer after layer towards the circumference. We could easily class their formation with the occasional deposition of calcareous matter among the other tissues, if Hodgson's opinion was correct, that they were first formed external to the veins, and afterwards made their way by absorption into the interior;[5] or if, according to Andral,[6] they were in general first developed in the middle coat of the veins, and then passed into their interior in the same manner as the false cartilages are formed, and pass into the cavity of the joints. Hodgson stands alone in his opinion; and we even find Mr. Langstaff,[7] upon whose authority he describes these bodies, inclined to believe that they are formed in the interior of the veins. The adhesions of these bodies to the inner coat of the veins by a fine membrane, as observed by Tiedemann and Cruveilhier,[8] would afford some countenance to Andral's opinion, were it not at variance with accurate observation; and we may suppose that this ap-

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1 Cyclopædia of Practical Medicine, vol. iv., article *Veins*. 1835.
2 Otto's Pathological Anatomy, translated by South.
3 Lobstein's Anatomie Pathologique.
4 Cyclopædia of Practical Medicine, article *Veins*.
5 Treatise on Disease of the Arteries, p. 521. 1815.
6 Andral's Anatomie Pathologique, tom. ii., p. 412.
7 London Medico-Chirurgical Transactions, vol. viii., p. 287.
8 Cruveilhier's Anatomie Pathologique, tom. ii., p. 71.

pearance was produced by the presence of the foreign body causing irritation and effusion of coagulable lymph.

Tiedemann[1] and, following him, Lobstein suppose them to be formed by a mechanical deposition of the calcareous matter contained in the blood, intermixing itself with the albumen of the blood. It appears to me that this opinion is unable to explain the manner of their formation in many cases, and consequently, can scarcely be regarded as the probable cause of their formation in those cases which seem to agree with this supposition. Mechanical deposition of the matters contained in the blood cannot, I think, explain their formation in those[2] cases where we find an osseous-looking deposit taking place in the centre of a coagulum, around which the fibrin arranges itself in concentric laminæ, increasing in density as we proceed to the centre, and where apparently those nearest the centre gradually assume this osseous appearance, which extends itself towards the circumference.

If these bodies resulted from mechanical deposition, could the earthy salts pass through the several dense laminæ of fibrin, and deposit themselves, apparently in certain proportions, in the innermost laminæ? If the presence of the earthy salts was the result of a deposition, would they not rather be found upon the outer surface of the most external lamina, instead of penetrating through it to reach the innermost? We cannot, at least in the present state of our knowledge on the subject, refer their formation to any chemical agency. Their formation is probably akin to the occasional hardening of tubercles into bony concretions, which is a process independent of any vascular organization of the tubercles themselves. It is

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[1] Journal Complementaire du Diction. des Sciences Médic., tom. iii.

[2] Cloquet's Pathologie Chirurgicale, and observations of Dr. Carsewell in article *Veins*, Cyclopædia of Practical Medicine.

also illustrated by the formation of the vegetations on the valves of the heart, as observed by Laennec and Dr. Alison, —also by the tubercular, purulent, or encephaloid deposits, sometimes found in the fibrinous clots within the heart and great vessels.

I can scarcely venture to hazard an opinion on this subject; but I must confess that I feel inclined to believe (however fanciful the opinion may appear to some physiologists) that the great resemblance of those bodies in their chemical analysis to the osseous texture, the manner of their formation, and their possessing in some cases physical properties similar to cartilage, can only be explained by supposing that they are the result of a process resembling the formation of the osseous tissue in the other parts of the body.

It is possible that when our knowledge of the spontaneous motions—or to express myself more correctly, motions without the agency of the contractile solids—observed in the circulating fluids of vegetables and animals becomes more extended, we may be furnished with a more satisfactory explanation of their origin.

Tiedemann and Lobstein are of opinion that these bodies are more frequently found in arthritic patients; and they believe that this may be accounted for, by supposing that the blood in those individuals may contain a greater quantity of earthy salts. The concretions, however, found in the joints of arthritic patients consist of urate of soda, a salt not discovered in the Phlebolites. In all the subjects in which I found them, there was no reason to suspect any arthritic affection; but having had very few opportunities of dissecting subjects who have suffered from gout, I cannot speak of their relative frequency. They are generally described as being deposited in the dilated veins; but we so often see the dilations corresponding to the varied sizes

of these bodies, that it appears probable that they are very frequently the consequence of their presence.

Since the preceding observations were written, I have found several Phlebolites in the spermatic or rather ovarian veins of the two last female subjects I have dissected. In one of these subjects we found them in all the different stages of their formation.

No. XVI.

THREE CASES OF PARTIAL HYPERTROPHY OF A PORTION OF THE ORGANS OF VOLUNTARY MOTION.

(FROM THE LONDON AND EDINBURGH MONTHLY JOURNAL OF MEDICAL SCIENCE FOR MARCH 1843.)

THE following cases of hypertrophy, or increased nutrition, confined to circumscribed portions of the organs of animal life, are of some interest in a physiological point of view.

CASE I.—*Increased Nutrition of the Left Thoracic Extremity.*—W. C., aged 15. General appearance that of a person enjoying moderately good health, and he was about the average size of boys of his own age. On examining the superior extremities after he had stripped himself, a very manifest difference in the relative bulk of these was at once apparent, the right being proportionate to the size of the lower extremities and to the trunk, and such as we would expect to find in a person of his age and physical powers, while the left presented all the appearances of belonging to a person of more advanced age and greater physical strength. This hypertrophy of the left superior extremity was not confined to any particular part, but was well marked in the hand, fore-arm, arm, and region of the scapula. This difference in bulk was undoubtedly principally dependent upon the difference of the relative size of the muscles and bones of the two extremities, and this was

equally well marked in all the bones, from the phalanges upwards to the clavicle and scapula, and in the various muscles attached to these. There was certainly no appreciable increase in the adipose tissue, and the cellular and cutaneous textures were probably developed uniformly with the muscular and osseous, but were certainly not carried beyond this. The skin of the hypertrophied arm presented a number of red patches, some of them of great size. One of these extended nearly over the whole of the scapula, leaving only the part over the inferior angle, of the natural hue. The other spots were principally placed over the outer side of the arm and fore-arm. The whole arterial system of the left superior extremity was obviously enlarged, and the pulsations of the subclavian, the axillary and all its branches, down to the digital, beat with great strength, and afforded a marked contrast to those of the opposite limb. He stated that he had a constant feeling of increased temperature in the left superior extremity. On placing a thermometer in each hand alternately, after he had been some time in a room moderately heated, it indicated a temperature of 77° Fahr. in the right hand, and of 86° Fahr. in the left. In the right axilla it was 98°, and in the left 100°. On another occasion it rose to 98½° in the right axilla, and in the left again to 100°. The following are a few of the measurements of the two extremities :—

	Right.	Left.
	Inches.	Inches.
Circumference of middle of arm, . . .	7	$9\frac{1}{10}$
An inch below elbow-joint, ...	$7\frac{5}{10}$	$9\frac{8}{10}$
Wrist, ...	$5\frac{4}{10}$	$6\frac{5}{16}$
From inferior angle of scapula to claviculo-scapular articulation, ...	6	$6\frac{6}{10}$
From inferior angle to middle of the spine of the scapula, ...	$5\frac{5}{10}$	6

He was unable to extend the fore-arm fully, and performed the movements of pronation and supination very

imperfectly. The only thing which appeared anormal about the elbow-joint was a well-marked projection of the olecranon. From the considerable pain occasioned by any attempts to extend the fore-arm fully, or perform any extensive movements of rotation and supination, he has seldom employed this extremity, except in those movements which are limited and require little muscular exertion. He stated that this condition of the elbow-joint, and the disparity between the size of the two limbs, have existed since his birth. The impulse of the heart against the chest was stronger than usual; and he stated that he had palpitation on ascending a stair. The apex of the heart was felt in its usual position. For the opportunity of examining the above case I am indebted to Dr. J. A. Robertson.

Case II.—*Increased Nutrition in one Toe.*—The subject of this observation was a female child, aged two years, healthy, and otherwise well-formed. In this child, the middle toe of the left foot projected about three-fourths of an inch beyond the great toe, and, taking its breadth along with its length, it equalled in bulk all the remaining four toes. The phalanges and metatarsal bone of the hypertrophied toe were felt to be of great size, and the foot had somewhat the appearance which we may suppose might be produced by transplanting the toe of an adult upon the foot of a child. The left foot was of great breadth compared with the right from the greatly increased thickness of the metatarsal bone, aided in all probability by an increase in the thickness of the interosseous muscles connected with this bone. The dorsal artery of the foot beat with increased force, but there was no appreciable difference in the two posterior tibial arteries as they pass behind the internal malleoli. The mother stated, that this disproportion between the two feet was present at birth. I am indebted to Mr. James Spence for an opportunity of examining the above case.

Case III.—*Increased Nutrition in the Thumb and First Finger of the Left Hand.*—This case is drawn up from notes furnished me by Dr. J. A. Robertson. The first exceeded the middle finger in length by about half an inch, and had twice its circumference. The thumb of the same hand was about a quarter of an inch larger than that of the right, and had nearly double the bulk. A thermometer placed between the forefinger and the thumb of the left hand rose from 2° to 6° Fahr. higher than when it occupied the same position in the right hand. The difference of temperature appeared to be greatest in cold weather, for in winter he was in the habit of warming the fingers of the right hand by placing them on the thumb and forefinger of the left. The radial artery of the left seemed to be double the size of that of the right arm, and felt more distended at each pulsation. There was no perceptible difference in the brachial and ulnar arteries of the two arms. The thumb and first finger of the left hand were observed to be longer at birth; and this difference in size, according to the statement of his friends, continued to increase up to the time of his death, which happened when he was 13 years of age. This boy, and the boy described in Case I., were nearly related.

In these three cases, the hypertrophied tissues possessed the property of nutrition in an anormal degree, and to a much greater extent than the other parts of the body. This appears to have been an original and not an acquired quality of these tissues—depending upon their original organization, and not upon the operation of extraneous causes after they had been formed. It must be sufficiently obvious, that no increased size or peculiar arrangement of the blood-vessels can account for this, since it is the greater or less activity of the molecular movements of nutrition and secretion which determines the quantity of blood in an organ, and not the mere quantity of blood which determines

the activity of nutrition and secretion. The heart drives the blood with equal force to all the organs of the body, and it is the extent of the manifestation of the properties of nutrition and secretion in each individual organ or tissue which regulates the distribution of that blood. Suspend the manifestation of the property of nutrition in an organ, and the force of the heart will be quite insufficient to continue the circulation of the blood through it to the same extent as before; in fact, the force of the heart, or *vis a tergo*, cannot keep the blood-vessels from diminishing in size, even to their obliteration, when the *vis a fronte* has ceased to act.

So active was this property of nutrition in the left thoracic extremity in the first case, that though this limb was little exercised, and consequently placed under circumstances highly unfavourable to the exercise of the function of nutrition, yet we have seen how much greater it was there than in the opposite and well-exercised limb. In the three cases the hypertrophied tissues were on the *left* side of the body.

No. XVII.

CASE OF MONSTROSITY BY INCLUSION, OCCURRING IN A BITCH.

(FROM THE LONDON AND EDINBURGH MONTHLY JOURNAL OF MEDICAL SCIENCE FOR SEPTEMBER 1845.)

During the winter of 1835-36, while assisting my friend Dr. Cormack in performing some experiments on the physiological effects of creosote, at the Edinburgh Police Office, upon some dogs doomed to destruction by municipal authority, the size of the abdomen of a small terrier bitch attracted my attention ; and immediately after its death, I opened it, in the expectation of finding it pregnant. The animal was in excellent condition, and, previous to the exhibition of the poison, in perfect health. On opening the abdomen, we found the uterus empty, of the natural size, and in every respect normal, the ovaries were plump and smooth on the surface; while two fœtuses, and masses of hair were lying among and on the surface of the abdominal viscera. I removed the whole of these viscera, and carried them home for more careful examination. The engraving, (Plate I.,) considerably reduced from the original drawing, gives an accurate representa-

tion of the removed parts. The previous history of the animal could not be ascertained.

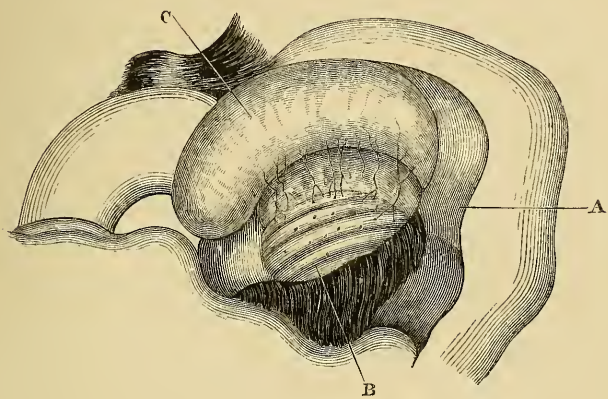

Fig. 1.[1]

One fœtus, the lower of the two, was lying obliquely in front of the small intestines, the left side of its body being directed towards the spine of the *including* animal, its abdomen downwards in the direction of the pelvis, and a little to the left side, the back upwards and to the right, the head looked upwards and to the left side, and was placed immediately below the larger curvature of the stomach, while its posterior extremity projected downwards, and to the right side. Two folds of the small intestines were closely tied to its dorsal, and other two to its anterior surface, and a portion of the large omentum, loaded with adipose tissue, adhered to its head. Its length from the point of the nose to the posterior part of the body, was $7\frac{2}{3}$ inches, the length of the head was $2\frac{1}{2}$ inches, the breadth of the upper surface, or that which looked towards the anterior wall of the abdomen of the *including* animal, was $1\frac{6}{8}$ inch at the shoulder, and $1\frac{1}{2}$ inch at the posterior part

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[1] A, Peritoneum. B, Ribs of fœtus. C, Intestine of the including animal.

of the body. It was much contracted at the loins, where two folds of the intestine were closely tied to it—one to the back, the other to the belly. The largest circumference of the head was found, at a subsequent part of the dissection, to measure $5\frac{1}{2}$ inches. The body was closely covered with hair, chiefly of a black colour. The head of the second or upper fœtus projected forwards below the pyloric orifice of the stomach, and first portion of the duodenum, while its body extended downwards behind the small intestines. When uncovered at a later stage of the dissection, it was found to be nearly of the same length as the other, but was decidedly less in its other dimensions. Several detached dark masses, most of them composed entirely of hairs, adhered to different parts of the intestines, to the surfaces of the liver, the lower surface of the diaphragm, the omenta, and the stomach. Four bones were also observed upon the anterior surface of the liver. One of these masses of hair, $1\frac{1}{2}$ inch in length, and half an inch in breadth, was placed in the right side of a fold of small intestine, passing between the posterior portion of the lower fœtus and the head of the upper fœtus, and was covered at the superior part by a process of the large omentum. After the arteries had been injected with a fine size-injection, I proceeded to the more careful dissection of the parts, under the impression that this was a case of *extra-uterine conception*, and it was only after satisfying myself that the fœtuses, the masses of hair, and the detached bones, were all placed *outside* the cavity of the peritoneum, or in other words, between the outer surface of the peritoneal sac and the surfaces of the viscera on which they were lying, that the notion of its being a case of *monstrosity by inclusion* occurred to me.

I believe, every one will admit, that if this case were one of extra-uterine conception, the fœtuses, hair, and bones, would have been placed on the inner surface of the peri-

toneum, or in other words, within the peritoneal sac; while it is agreed, that in cases of monstrosity by inclusion, the included fœtal parts are situated outside the cavity of the peritoneum.[1] I am fully aware, that nothing but the most satisfactory evidence will suffice to overcome the strong impression, I had almost said conviction, that must arise in the mind of the reader at the first view of the history of this case, that it is merely one of extra-uterine conception. I deem it therefore necessary to state, that the relation of these extraneous masses in the abdomen to the peritoneal sac was examined with the greatest care, and that the dissections which I made, not only satisfied myself, but every practical anatomist—and there were several—who examined them, that the foreign masses were all situated outside that sac. A few days ago, Professor Goodsir examined with me some of the masses of hair attached to the lower surface of the diaphragm, that had been left untouched in the previous dissections, and with me, had not a doubt that they were placed between the surface of that muscle and the peritoneum covering it: this serous membrane was then of its natural appearance, and in no way altered.

On cutting through the peritoneal covering of one of the folds of intestine adhering to the back of the lower fœtus, and separating it from the subjacent muscular coat, it was distinctly traced over the upper surface of the fœtus, and found to adhere to a thin serous membrane enveloping the fœtus. These two membranes were loosely united to each other for a short distance, while over the middle of the body of the fœtus they could with difficulty be separated. The passage of the peritoneal membrane over

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[1] Il est toujours situé en déhors du péritoine; c'est que son arrivée dans l'excavation abdominale, a précédé celle de l'intestin.—*Mémoire sur les Monstrosités dites par Inclusion, par Le Sauvage*, p. 30, Caen: 1829.

the upper surface of the fœtus was proved in another way. An incision was made through the membranes covering the head of the fœtus, and on raising them, it was seen that, on approaching the intestine, they divided into two layers—one of these becoming continuous with the peritoneal coat of the intestine, the other continuing closely to envelop the fœtus, and connect itself to the muscular coat of the intestine by areolar tissue. The layer enveloping the fœtus like a sac, was single, and presented the characters of a serous membrane.[1] The upper fœtus was enveloped in the folds of the small intestines, and was also covered by two layers of serous membrane—the one being continuous with the peritoneum, the other forming a sac enveloping the fœtus. Those portions of the walls of the intestines in immediate contact with the two fœtuses were uncovered with peritoneum. Numerous blood-vessels, chiefly continuous with those distributed in the sub-peritoneal cellular coat of the intestines, were ramified upon the outer surface of the sac enveloping the fœtus, and below the peritoneal coat. The large dark mass already mentioned as being $1\frac{1}{2}$ inch in length, and adhering to the outer surface of a fold of the small intestines, was placed between the muscular and peritoneal coats, the latter being elevated and very thin at the centre: the mass itself consisted of hair and bones, with blood-vessels ramifying through them. The patches of hair observed on the other parts of the intestine, the omenta and the stomach, were all placed completely outside the peritoneum, with the exception of a very few hairs, a small portion of one of the extremities of which projected through that membrane. A considerable mass of densely matted hair lay not only behind the gall-bladder, but even below the

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[1] According to Le Sauvage, (Opus. cit., p. 29,) Le fœtus inclus est enfermé dans un kiste séreux ; ce kiste est formé par son amuios.

fibrous capsule of the liver, and in contact with the parenchyma. On cutting through the peritoneum covering one of the masses of hair on the surface of one of the lobes of the liver, the hair was found to be imbedded in a depression in the substance of the liver, three or four lines in depth; some of them were intermixed with the parenchyma, and even a few projected from its substance as from the cutis. Four detached bones (*represented in the Engraving, and* C C C *in Fig.* 2.) were embedded in the fibrous capsule of the liver. One of these was a long bone with cartilaginous epiphysis, another was a rib, the other two were fragments; one of the two latter was of a dark colour. On cutting through the peritoneum at the posterior part of the liver, at its reflection upon the inferior surface of the diaphragm, and throwing back the muscle, a large and, for the most part, densely matted mass of hair was exposed, partly placed below the fibrous coat of the liver, and partly adhering to the lower surface of the muscle. Four well-formed claws were also found here, one of them placed at a little distance from the hair, and adhering to the fibrous capsule of the liver. (Fig. 2.)

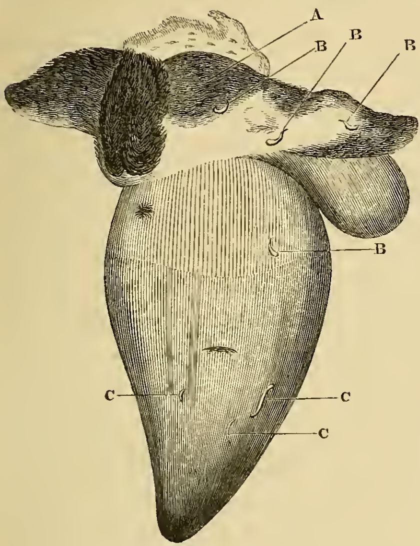

Fig. 2.[1]

[1] A, masses of hair adhering to diaphragm. B B B, claws. C C C, bones on surface of liver.

A pretty large artery, a branch of the gastro-epiploica dextra, ran downwards, and entered a dark mass, placed between the layers of the large omentum, and in front of the head of the upper fœtus. This mass consisted of hair, a fleshy-looking substance, and a claw. The artery, after having given several branches which ramified among these component parts of the mass, passed onward in its course among the layers of the omentum. On dissecting the folds of the intestine from that surface of the lower fœtus which was directed towards the spine of the *including* animal, those portions of the intestine in contact with the fœtus were found uncovered with peritoneum. One of these folds of intestine was in close contact with the ribs of the fœtus, which at this part were neither covered by hair nor muscle. The hair was collected in heaps along the outer edge of this fold of intestine, and a number of injected blood-vessels, perfectly visible by the naked eye, were seen passing from the muscular coat of the intestine, through the intercostal spaces in the fœtus. (Fig 1.) The only parts of this fœtus covered with skin were the head and posterior part of the neck, and the whole of the limbs, except at the upper portions. The rest of the surface was covered with great quantities of hair, from a quarter to half an inch in length, closely matted together. The back part of the neck, and the bones of the limbs, were well covered with muscle, but the superficial muscles of the neck, and the anterior and lateral walls of the chest and abdomen, were entirely wanting. The clavicles and ribs were well-formed and ossified, and, like the rest of the bones, were covered with periosteum; but they had no muscular fibres attached to them; and they were closely enveloped by hair. The long bones of the limbs were ossified in the centre, and cartilaginous at the extremities, and the tarsal and carpal bones were cartilaginous. The bones of the cranium were well-formed, while the spinal column was imperfectly ossified, its spinous

and transverse process being still cartilaginous, and the plates of the laminæ were neither united with the bodies of the vertebræ nor with their fellows of the opposite side. Several imperfectly developed muscular bundles were placed upon, and adhered to the inferior surface of the vertebral column, but none were found on its dorsal surface. The eyes were wanting. The orbits, nostrils, and mouth, were full of hair. The tongue and hyoid bone were present. The jaws were well-formed, but contained no teeth. The cavity of the chest contained a quantity of hair and fat surrounding a fleshy-looking mass, which consisted of a heart and a rudimentary lung. The œsophagus was not present; a few imperfect cartilaginous rings lying behind the heart were all that appeared for the trachea; and the bronchi were absent. The cavity of the abdomen was full of fat and hair, and all the chylopoietic and urinary apparatus were wanting, except an imperfectly formed stomach. The diaphragm was also imperfectly formed. The pelvis contained a soft mass, which could not be resolved into any of the usual viscera of that cavity. An aorta, with its large branches to the head and thoracic extremities, and a pulmonary artery, with its ductus arteriosus, were easily traced. The aorta divided at the lower part of the abdomen into two branches, from each of which an artery arose, and passed outwards through a mass of hair, to a very vascular, and somewhat spongy portion of intestine. The interior of the cranium and the spinal canal were full of a turbid greyish fluid, and the dura mater was present. The brachial, the lumbar, and the sacral plexuses of nerves, with their branches, were of their usual size. The nervus vagus of one side was present, and of considerable size, sending branches to the heart, rudimentary lungs, and stomach. The sympathetic nerves were traced along the whole length of the spine. Numerous small injected blood-vessels were observed passing, from the branches ramified on the sac

surrounding the fœtus, through the hair at different parts, to the surface of the bones and soft textures; but none were traced into their interior. The skeleton of the upper and smaller fœtus was in the same stage of ossification as the lower, but the ribs were dislocated and displaced. The only parts covered with integuments were the upper part of the head and the lower part of the limbs. The ribs, the bones of the shoulder and pelvis, the outer surface of the humeri and ossa femora, and the greater part of the spinal column, were not covered with muscle, so that the masses of hair enveloping them were in contact with the periosteum. The inner surface of the humeri and ossa femora, and the lower part of the limbs, were covered with the usual muscular bundles. The orbits, the nostrils, the mouth, the chest, and the abdomen, were full of hair. No viscera could be detected in the chest, abdomen, or pelvis. Scarcely any muscular fibres were attached to the vertebræ, and these bones could be separated from each other by a slight force. The cranium and spinal canal were full of a turbid greyish fluid, and the dura mater was present. The brachial and sacral plexures of nerves, with some of their branches, were present. Some branches of blood-vessels were also found among the muscles of the limbs. The serous sac which enveloped this fœtus was folded in at different parts among the masses of hair, and reached the surface of the bones and the muscles. A considerable number of the blood-vessels ramified on this membrane, and were in close contact with the tissues of the fœtus.

From the details given above, it will be perceived that the included fœtuses were monstrous formations; and in this respect the case agrees with the accounts given of all the well-authenticated examples of monstrosity by inclusion. We do not, however, attach much importance to this, as similar appearances may present themselves in an extra uterine conception. We rest the proof of the correctness

of the view we have taken of its nature entirely on the fact, very satisfactorily ascertained, that the included parts were all placed outside the peritoneal sac, and in positions sufficient, in our opinion, to prove, that they entered the abdomen of the including animal, before that membrane was formed. Had there been any imperfection in this part of the evidence, we would at once have concluded that this case was one of extra-uterine conception. Cases of monstrosity by inclusion, or of *fœtus in fœtu*, however improbable at first sight they may appear, are now, as is well known, placed beyond a doubt. They have been observed in boys, and in females at an age when impregnation of the including individual was out of the question. References to the greater number of the best authenticated of these have already been given in this Journal, vol. i., 1841, pp. 121-124.[1] The various attempts which have been hitherto made to point out the causes which produce this kind of monstrosity are far from being satisfactory. According to Isidore G. Saint-Hilaire,[2] the existence of monstrosity by inclusion in the lower animals is " à peine bien constatée pour une ou deux espèces."[3]

I may here take the opportunity of mentioning the two following cases, which I examined several years ago, and

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[1] *Vide* also Monthly Journal, vol. v., (1845,) p. 533.

[2] Histoire Générale et Particulière des Anomalies de l'Organization chez l'Homme et les Animaux, tom. iii., p. 316. Paris, 1836.

[3] Mr. Colman (*Vide* Baillie's Morbid Anatomy, p. 402, 4th Edition) discovered by dissection a tumour in the abdomen of a bay gelding, lying a little below the kidney, and of the size of a horse's testicle, containing two small molar horse teeth, and one incisor tooth, with a portion of bone attached to the last, resembling a jaw, with a quantity of fat and black hair. It could not be ascertained whether both the testicles of this animal had descended. Some physiologists consider it probable that such productions are the remains of a fœtus in fœtu. This and similar cases could not have been the result of extra-uterine conception, though it is quite possible that the formation of such masses in the ovary may be occasionally caused in that way. The supposition that they may depend upon abnormal nutrition, or too great excitation of the parts in which they are found, appears less satisfactory than that which refers them to monstrosity by inclusion.

which are not of sufficient interest to be made the subject of a formal and separate notice.

I received from a friend the uterus of an old woman, in the left ovary of which was a tumour about the size of a billiard ball. On cutting into this tumour we found its contents chiefly composed of hair and fat, and a fully formed bicuspid tooth adhered by its roots to the inner surface of the sac. No other hard part was present in the tumour. The preparation is now in my collection. In similar cases, the amorphous mass, according to Meckel, (*Journal Complém. des Sc. Médic.*, tom iv.,) is more frequently situated in the right than in the left ovary, the teeth generally have no root, and according to Isidore G. Saint-Hilaire, it rarely happens that the teeth are unaccompanied by bones.[1]

I received the uterus of a sheep a few minutes after it had been removed from the body. The animal was in excellent condition, and had been killed by the butcher for the market. It was supposed to be with lamb in the spring, but it did not produce any; and being in good pasture, it rapidly became sufficiently fat for use. The uterus, when removed, was considerably larger than in the unimpregnated state, and on opening it I found it full of bones, and skin covered with wool. All the bones of the skeleton were present, and the ossification was nearly advanced as far as is found in the lamb at the natural period of its birth, and all the muscles and even the ligaments had disappeared. The inner surface of the uterus was perfectly smooth, and covered with healthy mucous membrane. The parts had a very disagreeable, but not a putrid odour. The probable explanation of the facts of this case is, that the lamb had died, and instead of being expelled, had been

[1] "Il est rare que les dents trouvées dans les tumeurs ovariennes ne soient accompagnées d'aucun os. Baillie cite seul ou presque seul des faits de ce genre dans sa Morbid Anatomy."—Opus cit., tom. ii., p. 549.

retained in utero, and had been gradually reduced to the condition in which it was found at the death of the mother.

I have been informed of the particulars of a case very similar to the above. A cow was sold to a farmer as being with calf, and she presented the usual appearances of being in that condition. These indications, however, began to disappear, and the farmer obtained, in a court of law, remuneration for the loss he had sustained by the cow not producing a calf, as it had been warranted pregnant. The cow was fed for the butcher immediately after this, and when killed, the remains of a fœtal calf were found in the uterus. When this was made known the farmer was obliged to pay back the money he had received in the shape of damages.

Case in which both Kidneys were placed on the same side of the Spinal Column.—When in charge of the dissecting rooms in Old Surgeon's Hall, Edinburgh, I found that in one of the bodies which was being dissected by the students, the kidney was wanting on the left, and that there were two kidneys on the other side. The one was placed below the other, and the lower end of the upper one, and the upper end of the under one, were fused together. The renal artery supplying the upper kidney was given off by the aorta, near its usual origin; the one supplying the lower kidney arose from the aorta near its division into the two primitive iliacs. The ureter from the lower kidney passed across the mesial line, after entering the pelvis, so that these two tubes entered the bladder in the usual manner. The preparation is now in my collection. A case where the kidneys presented exactly the same appearance is described and figured by Dr. John Hunter, in the third volume of the *Medical Transactions of the College of Physicians in London*, vol. iii., p. 250. 1785.

No. XVIII.

CASE OF DISEASE OF THE SPINAL CORD, FROM AN EXOSTOSIS OF THE SECOND CERVICAL VERTEBRA.

(FROM THE LONDON AND EDINBURGH MONTHLY JOURNAL OF MEDICAL SCIENCE FOR MARCH 1843.)

George Sinclair, aged thirty, a flesher, was admitted into the Royal Infirmary of Edinburgh, on the 29th June 1840. Seven months previously he had been a patient in the hospital, when he was supposed to labour under an attack of rheumatism of the back of the neck, right knee, and ankle, and other parts of the body. After remaining in the house for three months he was dismissed convalescent, the pains still continuing in the parts specified. About eight days before the latter admission, he felt pain in the lumbar regions, which gradually extended up to the left side, and then to the back of both thighs and arms. The pain in the neck increased, and he continued in this state until re-admission, when he complained of pain in the back of the neck, more severe towards the right side, and his head was permanently turned towards the right shoulder. He had pain in both arms from the shoulder to the elbow, and the forearm and hand felt numb and stiff. He had also

pain in the back, extending up to the left side, and down to the backs of the thighs. He complained much of cold sweats, especially at night. The pulse was quick and small. The bowels having been opened by medicine, he was ordered doses of Dover's powders, and was leeched over the back of the neck.

July 2d.—He complained chiefly of pain of the shoulders on motion, and stiffness and pain on motion of the neck. He was ordered tincture of colchicum ; and next day leeches were again applied to the neck.

6*th.*—The neck was easier ; but the pain in the arms was worse. The bowels were rather loose.

7*th.*—He had an indifferent night. Yesterday his stools were frequent; but to-day he has only had one. The pains are no better; pulse 46. The colchicum was discontinued, and the extract of conium maculatum prescribed.

9*th.*—The pulse was fuller—about 50. He complained of more pain of the neck. After consultation between Mr. Syme and Dr. Christison, it was agreed to treat the case as disease of the second cervical vertebra; and, in accordance with this view, the actual cautery was applied to the back of the neck.

11*th.*—He has had sickness and headach since yesterday afternoon, when the cautery was applied; his sleep has been disturbed; pulse 54; skin warm; tongue moist. The hemlock was discontinued.

12*th.*—He has slept better since the application of the cautery; the skin is warm and moist. The pain and numbness of the left hand were better; the right hand was much the same.

22*d.*—The neck was on the whole better, and also the left hand; the right arm was weaker, with more pain of the hand; pulse natural. He took extract of conium last night, and slept better. It was ordered to be repeated.

August 10*th*.—He had more pain in the right side of the head for the last few days, which, in the report of this date, is described as more aggravated. Eight leeches were applied to the neck.

12*th*.—He complained of increased headach.

His symptoms continued much the same until the beginning of October, when febrile symptoms supervened. The report on 2d October is, that he complained yesterday of great increase of headach, and that twelve leeches were applied to the neck, which bled well and gave relief. On this date the pulse was 116, and firm; breathing hurried; tongue furred and moist, with bad taste in mouth; no appetite; much thirst; no local pain, except of head; some delirium last night, but is quite coherent at present; he has had one watery stool; his head was shaved, and twelve leeches were applied to the temples, which bled well. A dose of castor oil was prescribed, and he was ordered to take a dose of the aq. acet. ammoniæ every two hours.

3*d*.—The headach was much relieved; pulse 128, of tolerable strength; the respiration was hurried; and there was some cough, but no pain of chest; florid elevated petechiæ more distinct than yesterday; his sleep has been disturbed; he has had several dark and liquid stools.

There having been little change in the symptoms, no report was made until the 6th, when the respiration was described as hurried, and rather oppressed; pulse quick and small; hands cold, and the tongue rather dry; the stools were now passed in bed; there was slight livor of the face. He was allowed four ounces of wine.

7*th*.—He appeared rather better. He complained of thirst, and the mouth was dry.

8*th*.—The petechiæ were much more distinct; and there was considerable swelling, with very large vibices, of the

right arm, but without pain, and attributed to his lying upon it; the arm moved with difficulty; pulse 128, of tolerable strength; one stool daily; seems quite coherent. The allowance of wine was doubled. The pulse became more feeble, and he died on the 14th.

16*th.*—*Autopsy.* The lungs were considerably congested at their posterior and middle parts; the heart was healthy; the abdominal organs were healthy; the spinal cord was compressed opposite the upper part of the second cervical vertebra by a conical exostosis, about one-third of an inch in length, growing from the posterior part of the root of the odontoid process. This exostosis had produced a marked depression in the centre of the spinal cord, immediately below the decussation of the pyramidal bodies. On cutting into the cord at this part, the whole of the central portion was found to consist of a soft reddish-brown pulp. The only part of the cord which here appeared healthy was a thin layer on the lateral portions, varying in thickness in different parts, but in some places not thicker than one line. The space between the inner surface of the transverse ligament, and the point of the odontoid process was filled up with a dense cellular tissue.

This patient was able to rise up to stool during the attack of typhus fever which preceded his death.

In this case we find, that though the whole of the central portion of a part of the spinal cord was in a state of *ramollissement*, from the effects of external pressure, the portion of the cord thus altered could nevertheless transmit downwards the motive influence of volition, and of the excito-motory respiratory movements, and convey upwards those impressions which excite sensations. In this respect it is similar to a case recorded by M. Rullier in Magendie's *Journal de Physiologie*, tom. iii., p. 173. In both cases

the severe pain felt in the upper extremities, in all probability was dependent upon the morbid changes going on near the roots of the sensiferous nerves distributed in the pained parts; for, as is now well known, the painful sensations arising from the excitation of the trunk of a nerve are referred to the extremities of its expanded branches. The presence of very severe pain in a part is thus frequently no proof that the nerve has either become more excitable than usual, or that any unusual excitant is acting upon its filaments at the part in which the pain is felt, but may depend upon changes going on in the trunk of the nerve considerably nearer the central organs of the nervous system. The facts of the case which we have described appear to be in accordance with the results obtained in experiments made by Desmoulins and Magendie upon the lower animals. It is stated by these authors, "that the passage of a stilet along the interior of the whole length of the spinal cord does not affect, in any marked manner, the sensibility or the movements of the animal. This fact implies that all the parts directly destroyed by the stilet, and all the circumjacent parts torn and bruised by it, exercise little or no influence upon these phenomena. From this it may be concluded, that since these parts do not act under the circumstances induced by our experiments, they do not act in the ordinary and healthy condition of the body. Impressions are not then transmitted by the whole mass of the spinal cord, but by the parts on its circumference."[1]

Whether or not we feel inclined to adopt these inferences drawn by Magendie and Desmoulins from the ex-

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[1] Anatomie des Systèmes Nerveux des Animaux à Vertèbres, Deuxième Partie, p. 551. *Vide* also Magendie's Journal de Physiologie, tom. iii., p. 154.

periments referred to, they present additional claims to our notice from the case of Rullier, and from that related above.

Various cases are on record, in which even the whole thickness of the spinal cord has been destroyed by injury and disease, without being followed by loss of sensation and volitional movement in the parts supplied with nerves attached to the portion of the spinal cord below the part destroyed.[1] These cases are, however, so exceedingly at variance with all the other facts and observations connected with the injuries and diseases of the spinal cord, that we feel irresistibly led to believe that some circumstances must have been overlooked, or some error committed, in observing them. It is possible that in some of these cases part of the cord still remained entire, and was torn through by the violence employed in laying open the spinal canal; while in others reflex muscular movements may have been mistaken for volitional. There can at least be no doubt that this wide discrepancy does not arise from any capricious alteration of the laws by which the healthy and diseased actions are regulated, but from the inherent difficulties attending all observations upon the phenomena presented by the living body, and, above all, by a want of due precaution on the part of the observers themselves. We could more readily believe that a person may continue to retain voluntary power and sensation in the lower extremities after the spinal cord has become altered by disease, provided that those changes in its texture took place slowly and gradually, than after disruption or rapid destruction of a portion of the spinal cord, for it may be pos-

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[1] Case by Dessault in his Journal de Chirurgie; Velpeau in the Archives de Médecine for 1825; and Olivier in his Traité des Maladies de la Moëlle Epinière, tom. ii., p. 368.

sible that though part of the natural texture of the spinal cord appears to be changed, it may still continue to perform some of the functions of the nervous matter, in the same manner as the heart has been stated to retain its contractility to a certain extent after the muscular tissue had been in a great measure replaced by a fatty substance. We cannot, however, place much reliance on this analogy in the present state of our knowledge.

No. XIX.

REPORT ON THE EPIDEMIC FEVER OF EDINBURGH.

AN ACCOUNT OF THE SYMPTOMS AND TREATMENT, BY DR. HENDERSON; AND AN ANALYSIS AND DETAILS OF INSPECTIONS AFTER DEATH, BY DR. REID.

(FROM THE EDINBURGH MEDICAL AND SURGICAL JOURNAL, NO. 141.)

The fever described in the following pages has been epidemic in Edinburgh for three winters, gradually declining as the spring advanced, and almost disappearing in the summer,—that of 1836 excepted, in the course of which a considerable number of cases occurred, and it was then that the disease began to prevail extensively. This account of the disease has been constructed from cases which occurred during its last irruption, dating from the end of October 1838 to the middle of June 1839. Within that period nearly 200 cases came under my particular care; and it is from these, although they form but a small proportion of the whole number admitted into the Royal Infirmary, that the account is derived.

The difficulty of forming a satisfactory diagnosis has been felt in many instances in which the disease has terminated within six or eight days,—without scope having been afforded for the development of that group of symp-

toms, which in the second week, more especially, of continued fever distinguishes it widely from every other disease. The very continuance of a febrile state for many days, without apparent dependence on any particular local disorder, is, when other circumstances are wanting or insufficient, a material assistance in guiding to a correct opinion of the nature of the malady, and when this, as well as the peculiar symptoms of continued fever, is absent, as it is in the slighter cases, the diagnosis is very much embarrassed. The symptoms which attend the invasion of the fever, and the subsequent condition of the pulse, the skin, and the tongue, during the first week, were found to be not uniformly of the same character in cases which were, notwithstanding, eventually ascertained to be cases of continued fever. Yet the symptoms of invasion, the state of the pulse, the skin, and the tongue, were the principal data afforded in the slighter cases, and in the earlier stage of the disease.

To exclude those slighter cases, to deny their intrinsical affinity to the other and severer cases, and to constitute them a different species of fever, are measures which do not appear to be justifiable, nor are they consistent with the custom of some very high authorities. Dr. Bateman, in reference to this subject, says in his account of the fever epidemic in London—" Its character is greatly varied by the different circumstances in which it occurs, by the age, constitution, and previous health of the patient, by the intensity of the exciting causes, by the situation and season, and by early neglect or mismanagement; but it is not more varied than other febrile diseases, the small-pox, for instance, or scarlet fever, under similar circumstances; and examples of the most distinct modifications which it undergoes are often observed in individuals of the same family. Thus in the instance of a man and his wife, who were brought to the House of Recovery together, the former

was affected with the mildest symptoms of fever, which scarcely confined him to bed, and terminated in a speedy convalescence; while his wife was lying in a state of stupor, her skin covered with petechiæ and vibices—in a word, exhibiting the most formidable symptoms of the worst form of typhus. Yet these extreme degrees of the disease manifestly originated from the same cause; and it would be equally unphilosophical to account them different kinds of fever, and give them distinct generic appellations, as in the case of benign and confluent small-pox, which are generated in like manner from one contagion." If the presence of musculæ, red spots, or typhoid eruption, be considered as conclusive of the nature of the disease, though not necessarily a symptom in all cases of continued fever, then I can have no hesitation in deciding that this fever may run its course in a few days; for instances of convalescence have occurred to me on the seventh and eighth days, in which the eruption had existed, without which the diagnosis would have been perplexed as in other slight cases. The eruption, however, is not a usual character of slight cases; but, as may be observed in the account to be given of it, corresponds very generally with the severity of the case, and commonly becomes scanty and imperfect in the less severe cases, while in those of a slight nature it rarely appears at all, though these may have come from the same locality, and even from the same family as the others.

In the frequent absence of pathognomic symptoms in the early stage of fever, and, consequently, in such cases as do not extend beyond this stage, supposing it to comprehend the first seven or eight days, it has been thought proper, in order to determine the nature of those cases, and to exclude trivial disorders, bearing no affinity to continued fever, from being classed along with it,—to require in all the milder cases a correspondence with the majority of the more

protracted and severe, in the following respects—in the symptoms of invasion, and in the state of the pulse, tongue, and skin, (the eruption excepted,) as exhibited in the early stage of the latter class of cases. The symptoms which characterized the period of the invasion of the fever were very generally headach, more especially frontal headach, pain in the lumbar region, in the lower limbs, and not unfrequently in either the chest or abdomen; these symptoms were in many preceded for a day or two by languor, dejection, anorexia, and transient chilliness, and accompanied by sensations of cold, in the back particularly, by impaired muscular strength and mental activity, and, as ascertained in a few cases, by a soft, rather small, and accelerated pulse. For several days after this, even in the milder cases, pains in the head and back, and general soreness continued, while the skin became preternaturally hot and dry, the face flushed, thirst great, the tongue coated, and the pulse more accelerated, ranging in this early stage both in mild and severe cases, indifferently, from 90 to 120 in a minute, seldom much developed in size, commonly moderate in strength, or both small and tense. After the lapse of six or eight days, those symptoms became in the mild cases much modified, the tongue first beginning to clean, sleep becoming calmer and more prolonged, the skin moist and cooler, the evacuations, if previously dark, of a better colour, with other ordinary symptoms of convalescence. In those cases which did not begin to improve so early, the symptoms which have been detailed began sooner or later to be accompanied or succeeded by those indicative of greater muscular prostration, a more disordered state of the secretions, indications of cerebral disturbance or oppression, and impaired vigour of the circulation; while it was very generally among them, in this more advanced stage, that the skin attained its highest temperature. Convalescence in those cases was gradual, as in the others;

sudden improvement, and speedy recovery, in connexion with abundant secretion from the skin, bowels, or kidneys being extremely rare. In one or two cases spontaneous purging, or copious perspiration, were harbingers of convalescence. More commonly the change was gradual, denoted more by improvement in the nature of the intestinal evacuations than in their quantity, and by decrease of heat, and gentle moisture, than by abundant perspiration.

In giving a more particular account of the epidemic, I have had recourse, as far as the records enable me, to a numerical analysis of the most important symptoms; a method which, though it gives no definite delineation of any particular case, is yet best calculated to impart a general comprehension of a prevalent disease. The same plan is adopted in stating the results of treatment.

1. *Circulation.*—Regarding the state of the circulation, I need not enter into particular details, because its most important feature, strength, will be better understood from what is stated concerning the results of the different plans of treatment, than by the vague terms in common use to denote the sensation imparted by the pulse, concerning which also there is not unfrequently considerable difference of opinion. In frequency the pulse, except in children, rarely rose so high as 140, but in the worst cases; in one adult female, it beat 156 times a minute on the thirteenth day, although no other unfavourable symptom was present, and the case ultimately did well. The common range in the height of the disease was from 110 to 130, the higher amount of frequency being usually the result of a gradual, but not regular, increase from day to day, from the commencement. In a very few cases it rose little above the natural standard.

2. *State of the Head.*—The head was almost universally the seat or source of symptoms in the course of the fever. These consisted of every diversity in degree, from the

slightest uneasiness to severe pain, and from mere oppression, languor, and confusion of the faculties, to stupor and violent delirium, with vociferation, and attempts to get out of bed.

Headach.—Among 132 females, the state of the head in regard to pain was carefully noted in 108 cases. Of these, 104 were affected with headach in the course of the disease. Ninety-two of them were known to have had headach at the commencement of the illness; in twelve, the earlier symptoms were not fully ascertained, but the existence of headach was recorded at an after period; in four cases, there was no headach at any time. Of fifty-one cases among the male patients, in which particular notice was taken of the state of the head, forty-nine had headach.

Delirium, &c., in female patients.—In twenty-five of the 132 females there was a notable degree of delirium; in thirteen much stupidity and indifference to external circumstances approaching to stupor; in fifteen deafness; in two considerably impaired vision at the commencement, with intense headach. The remaining cases had either no degree of those symptoms, or, as was commonly the case, were affected with inconsiderable confusion or sluggishness of mind, or muttering and moaning in sleep. Carphology, *subsultus tendinum*, and convulsions were rare; the two former mostly in the cases which ultimately were fatal, the last only once, and in a fatal case. Of involuntary evacuations no precise records have been kept; but, except towards the close of the fatal cases, there were few who could not give notice to the attendants, or express uneasiness when about to evacuate the bladder or bowels. Retention of urine occurred in three, who had no other very unfavourable symptom.

Date, persistence, &c., of these symptoms.—In a few cases headach in some degree continued throughout the course of the disease. It very generally persisted throughout the

first week, means having been resorted to for its removal in few before the lapse of that period. The average duration of this symptom, derived from twenty-nine cases, in which the date of its final cessation is specified, was ten days.

The other symptoms pertaining to the head belonged generally to a later date. Delirium began on an average of twenty-one cases about the eleventh day; in only three cases before the ninth; in the majority between the eleventh and fourteenth. Considerable oppression, approaching to stupor, was later still usually in presenting itself, the average period being the fourteenth day.

The disorder of the mental faculties not only occurred at a date posterior to the average term of the headach, but held no particular relation to the pain. Thus, on selecting cases with the severest degree of headach, it appears that in twenty-nine such cases there were twenty-one that had at no period of the fever any cerebral disorder worthy of being noted, while eight had delirium. On the other hand, there were eleven cases of considerable delirium, in which the headach had been of the ordinary kind. In seven cases of delirium the headach had previously ceased; in four it was still present in a slight degree; in several of the others severe headach and delirium co-existed. The violence of the delirium did not seem to bear any correspondence to the pain, for of six cases of much delirium, with shouting and attempts to get out of bed, and in which the state of the headach could be learnt, in five the pain was slight or gone, in one only severe; while in two cases severe headach co-existed with moderate delirium.

Deafness came on about the thirteenth day commonly; in three, however, it was among the earliest symptoms, and accompanied at the commencement with severe headach. In five others at a later period there was some degree of headach along with the deafness. In six cases the deaf-

ness was associated with delirium; in one with a tendency to stupor.

Delirium, &c., in male patients.—Among sixty-six males there were twenty-three cases of delirium, and three of oppression approaching to stupor. Deafness existed in a smaller proportion than among the females; it has been noted in only one case. There was no difference worthy of remark between the males and the females in regard to these symptoms, with the exception of certain circumstances connected with the delirium. The proportion of cases which possessed this symptom was larger among the male patients, amounting to more than one-third of their number, while among the females it was less than one-fifth. The average date at which delirium began among the former was earlier than among the females, or on the tenth day; in nine cases on or before the ninth day. In two cases it had the character of extreme excitement, or furiosity, of which no case occurred among patients of the other sex.

Tremors of the tongue and hands were not uncommon; subsultus in any considerable degree was confined to the fatal cases. Convulsions occurred in one person, a boy of fourteen years old. After headach, with tendency to stupor, for several days, he was seized with convulsive contraction of the upper extremities, convulsive motions of the lower, insensibility and strabismus,—the fit lasting about an hour, and recovery succeeding without relapse.

3. *State of the Digestive Canal.*—*Of the tongue.*—In 100 cases among the females an exact account was kept of the state of the tongue. It very early became covered with an increased and altered secretion, white, yellow, or ash-coloured, viscid, and adhering to the surface, becoming commonly thicker and darker as the disease advanced. A dry state of the tongue began chiefly in the second week of the fever, and continued for the most part, without change,

until, along with other symptoms of convalescence, the tip and edges assumed a moist and clean appearance, which gradually extended to the rest. The dryness was often confined to the centre of the tongue, extending in a brown streak from the point backwards. In seventy-three cases the tongue was dry at one period of the disease; in twenty-seven it continued moist throughout, and in several of these was at the same time almost quite clean—in cases too which were not always mild, for one with the tongue in this clean and moist state died on the thirteenth day.

State of the Evacuations, &c.—The observations on the state of the tongue and the following on that of the evacuations are applicable, generally, to patients of both sexes. The intestinal evacuations in their most disordered state, were very dark, slimy, and offensive; and in a more or less considerable degree they possessed those characters in almost every case, a few only of the mild, and one or two of the protracted cases, having had throughout stools of a light yellow colour. In many at the time of admission into the hospital the evacuations were consistent, though otherwise disordered: subsequently, either by the use of purgatives from day to day, or, as happened more especially in the advanced stages of the disease in a good many cases, by a spontaneous laxness of the bowels, the evacuations were thin, as well as offensive and dark-coloured. The laxness which occasionally occurred, and which I have not included in the account of diarrhœa, rarely amounted to more than four stools in twenty-four hours, and this state was not often sustained for several successive days, but was only occasional, and every such case had been, at a previous period, freely purged by medicine, and was very commonly subjected to the renewed operation of cathartics, notwithstanding the easy state of the bowels, as long as the discharges were of an unhealthy quality. A change from the unhealthy condition of the evacuations to a yellowish and

lighter colour, and less fœtor, very commonly accompanied, or immediately preceded, the indications of convalescence, or rather constituted one of the earliest symptoms of convalescence. In no instance was there a discharge of consistent fæces towards the conclusion of the fever; in one case only was there discharge of blood from the bowels.

Among the female patients at the time of their admission there were thirty-one cases in which the bowels were costive, and seventy-three in which they were in an easy state. It was ascertained that twenty-eight of the latter had taken purgative medicine a short time previous to their admission. In all of these evacuations were easily and regularly maintained subsequently by moderate doses of medicine—some having need of medicine for this purpose less frequently than others, owing to the condition of the bowels already noticed. In five cases there was spontaneous diarrhœa, or evacuation of very liquid stools, six or eight times, or oftener, in twenty-four hours. In two of these cases the diarrhœa occurred at the beginning of the fever, and was not subsequently remarkable; in one it occurred on the sixth day, but afterwards the bowels required medicine to move them, and the abdomen was neither tense nor painful; one had copious purging on the nights of the eighteenth and nineteenth days of the fever, succeeded by convalescence; in the remaining case there was diarrhœa at the commencement, which was not remarkable again till the close of the disease, and was then accompanied by epigastric tenderness.

In forty-five men the state of the bowels was noted at the time of their admission. In nineteen the bowels were bound; in fifteen they were open from medicine, and in eleven moderately without medicine. Three cases had subsequently diarrhœa—all very late in the disease. One case was that of a child, in whom towards the close of the fever, a degree of acute gastro-enteritis, with tension and tenderness of the belly, supervened; the other two cases had dur-

ing the convalescence from fever with the ordinary symptoms, some bloody mucus in the evacuations, accompanied by pain in the abdomen, and some recurrence of pyrexia.

In twelve cases—all women—there occurred nausea and vomiting, chiefly at the beginning of the fever. Five of these had also pain and tenderness of the epigastrium.

4. *Pain and tumidity of the Abdomen.*—Forty-six cases among those of both sexes had abdominal pain and tenderness. In twenty-one the pain existed in the epigastric region; in eleven it was not confined to any particular part of the abdomen, and was unattended by any other remarkable disorder of that region; in eight a degree of general tenderness of the abdomen co-existed with tumidity and tension, in only one of which was there diarrhœa. Two cases had pain confined to the left iliac region, three to the left hypochrondrium, one to the right side of the umbilicus. Tenderness of the right iliac region was remarked in none. In a good many cases besides those specified, very firm pressure on the abdomen seemed to produce uneasiness, but not in a degree worthy of being recorded.

5. *State of the Respiratory Organs.*—The respiratory acts were in a good many cases much accelerated in the advanced periods of the fever, in some to thirty-six or forty times in a minute, without any sign existing of disease in the lungs or bronchiæ. Among the men, there were twenty-five cases with symptoms of a disordered state of the thoracic viscera. Five had cough in a moderate degree without expectoration, two had merely pain in the chest on full inspiration, or on coughing, and eighteen had symptoms of bronchitis. These consisted of cough, expectoration, in some a degree of pain, and the usual catarrhal rattles. In three of these cases the bronchitis had been present for several weeks before the fever. In eight the bronchitic affection came on within the first seven days, and for the most part with the first symptoms of the fever. In the remainder it

attracted notice later; in several, not till convalescence from the proper symptoms of fever had begun, and then had the effect of prolonging a state of pyrexia and debility. In only seven cases were the expectoration very considerable, and the bronchitic rattles general. The bronchitis was usually subdued between the eleventh and eighteenth days.

Forty-eight cases among the women had symptoms of disorder in the chest. In fifteen of these they consisted of cough with little or no expectoration, and of pain on full inspiration without the other symptoms. Thirty-three cases had smart, or severe and general bronchitis, often with pain in some part of the chest, or feelings of tightness and dyspnœa, with accelerated respiration. In all these there was expectoration, in many to a very considerable amount—from ten to twelve ounces in some in the course of a-day. In eleven cases the bronchitis came on in the first week of the fever at different periods from the commencement; and in a few it had existed for weeks previously. In six cases it did not occur till between the thirteenth and nineteenth days. Four of the females died of the bronchitis; one of them had habitual bronchitis and emphysema of the lungs. Pneumonia did not occur in a single case of either sex. One female in an advanced stage of fever with bronchitis presented on the right side posteriorly an impaired sound of precussion, and small mucous or subcrepitous rattles at the same place; but next day the sound of percussion was natural, and the symptoms generally improved. It was probably inflammation of the smaller tubes, with congestion of the lung. Of the seventy-three cases of both sexes affected with symptoms of thoracic disease, many were affected with some of those symptoms which were referable to the abdominal viscera.

6. *State of the Skin.*—Typhoid eruption. One hundred and thirty cases of both sexes were specially inspected with reference to this eruption. In one hundred and eight

cases the eruption was found; in twenty-two it was not found; six of the twenty-two cases were not admitted till between twelve days and three weeks from the beginning of the fever; therefore, as will appear from what follows, it cannot be concluded that they had not had the eruption at an early period. Of the sixteen cases in which no eruption existed at any time the greater number were slight cases; one only could be termed a rather severe case, extending to the fifteenth day. This case was not traced to contagion. Several of the other cases, both adults and children, in the place of the typhoid eruption, had petechial stains, which, as is well known, are distinguished from the eruption by their not being affected by pressure, while the latter disappears for a short time when pressure is made.

Appearance of the Eruption.—The eruption appeared in four different forms, and the depth of colour varied in different cases.

1. The most common figure assumed by the eruption was that of eliptical, and, more rarely, circular spots, not raised in general above the surrounding surface, and from two to four lines in greatest diameter. Spots of this kind were either few and widely scattered or in various degrees of abundance; when the most numerous were so closely approximated, that three or four might be included within the circumference of a shilling. In the greater number of cases, however, though abundant, they were scattered more widely.

2. A second form of the eruption consisted of punctuations, as if produced by the point of a pen dipped in blood. These punctuations varied also in number. When the most numerous, they covered the surface more completely than the first form, so much, indeed, in some cases, that at a little distance, the integuments appeared of an almost uniformly florid hue at the parts where the specks were the most crowded. Generally they were less abundant than this, and gave an irregularly freckled aspect to the skin.

3. In one case the eruption consisted almost solely of large and intensely florid patches, half an inch in extent, and irregularly shaped. These were a little raised above the surrounding surface. Patches so large were more commonly noticed in very small number, mingled with some other form of the eruption.

4. The fourth distinct form of eruption was the papular. There were but few cases in which papulæ alone existed. In general they were scantily disseminated through an eruption of another form. There was nothing different in their appearance from other papulæ. They were of moderate size, like those of the second day of variola, and it was in this form of the eruption alone that desquamation of the cuticle distinctly occurred. They were easily distinguished from elevated elliptical spots, by their decided acumination.

It was not very unusual to find several of these forms of the eruption mingled together, more especially the first and second. Of fifty-three cases in which the figure of the eruption is specially detailed, thirty-two pertained to the first form; nine of these were closely spotted; seven moderately; sixteen loosely; ten were punctuated, six profusely, four moderately; one presented large patches; five were papular; and five were mixed.

Colour.—The colour of the eruption was influenced a good deal by what was natural to the skin. In persons of dark skin, it was commonly dusky or brownish red. On very fair skins, it was sometimes, especially in the beginning, a very light rose colour, deepening usually as the disease advanced. In general it was such as might be represented by a mixture of vermilion and lake, having a preponderance of the former. In a few the tint was somewhat purple; and this colour could be assumed by an eruption that had formerly been scarlet. This was particularly noticed in cases that became suddenly worse, and was a very unfavourable symptom.

Situation.—The trunk of the body presented the eruption much more abundantly and distinctly, in general, than the extremities. The upper part of the chest, the region under the mammæ, and the sides of the abdomen, were the most favourable situations for observing it. In not a few, especially when the eruption was abundant elsewhere, the extremities, particularly the haunches and thighs, were profusely mottled as with a closely set rash. The face was not commonly affected, or the eruption was with difficulty distinguished from the flushing common in the disease.

Time of its Appearance.—The exact period of the fever at which the eruption first presented itself I have been able to ascertain in but very few cases; a circumstance to be ascribed chiefly to the lateness of the general dates of admission into the hospital. In twelve cases only has the first appearance of the eruption been witnessed. In two it happened on the third day, in one on the fourth, in two on the fifth, in three on the sixth, the remaining four severally on the seventh, ninth, eleventh, and thirteenth. The three last dates must be esteemed as rarely witnessing the first appearance of the eruption; for although many more patients were admitted after the first week of the fever was over, than before that event, the eruption could be detected making its first appearance in only three after the eighth day. The other cases had the eruption in process of development at the time of admission; about eighty cases admitted between the third and ninth days of the fever had the eruption already in progress.

Development of the Eruption.—The eruption was commonly developed gradually in the course of several success sive days, before reaching its greatest amount. At first the spots were both few and faint in colour, or, if numerous at the earliest periods, still feebly coloured. Their subsequent increase in number and distinctness was usually progressive from day to day, and they attained their complete

development after the lapse of periods various in duration. Thus while some had the eruption perfected on the sixth, or even on the fifth day, others known to have had some eruption on the fourth or fifth, did not present it in a complete state before the eighth or the tenth; and in one case, with eruption noticed on the seventh, it was not completed before the fourteenth. This last would appear from the following data to be a rare exception to the general rule. The dates at which the eruption attained its completion have been noted in forty-two cases. In thirty-five of these this event happened on or before the tenth day; in twenty-five between the eighth and tenth inclusive; in the remainder between the eleventh and sixteenth days. While the eruption generally occupied, as appears from those facts, four or five days (supposing it to have usually commenced between the fourth and sixth) in reaching its greatest degree, there were some instances in which the transition from a faint and scanty condition to the fullest distinctness and abundance took place rapidly, even in a single day. In one case the report on the ninth day is, "skin since yesterday abundantly covered with a florid eruption of small spots of various sizes;" the increase subsequently was trivial. The eruption was not necessarily abundant, though occupying several days in being completed, any more than it was scanty, though it became perfect rapidly. Thus, to give an extreme case in illustration of the former statement, on the seventh day, one florid spot was noticed on the trunk, on the eighth, three, and on the ninth, six, besides the few that were dispersed on the limbs. In some cases a moderate eruption continued for several days without increasing, and then became more abundant, and in others an eruption that had begun to fade has again become distinct, and even more abundant than formerly. In one after having entirely ceased, it came back in a slight degree.

Persistence of the Eruption.—The continuance of the eruption in a state of completeness was subject to considerable diversity. The date of the first indications of its decline was noted in forty cases; in eleven before the tenth day; in twenty between the tenth and twelfth, inclusive; in one as late as the seventeenth. This, compared with the dates at which the eruption was commonly completed, will leave from two to four days as the ordinary duration of the eruption in its complete state. In some cases, however, the limits of the completed eruption much exceeded this. Five and six days have been observed as the term of this state in some instances. On the other hand, in a few, the whole course of the eruption was very rapid; three days in two cases witnessed its origin and decline. The treatment appeared to exert some influence in shortening the course of the eruption. I have observed early blood-letting succeeded on the fourth day by its decline.

That the spots throughout the whole duration of an eruption were the same, I ascertained in a few cases, by surrounding a number of them with ink. In one case, those spots remained six days, in another seven, undergoing the same changes, merely, as the general eruption was doing.

Fading of the Eruption.—In fading, the eruption occupied an uncertain period. Of twenty-five cases in which the date is recorded at which the eruption had entirely disappeared, in nineteen this occurred between the thirteenth and eighteenth days; in one only earlier than the tenth. In the majority the spots continued to fade day by day, until after the lapse of four or five days, there remained but the faintest traces of stains in the skin. The whole was gone commonly about, or soon after, the close of the second week of the fever. From these facts it follows, that the whole duration of the eruption occupied commonly nine or ten days.

Progress of other Symptoms compared with the advance of Eruption.—In twenty-two cases of copious or considerable eruption, there was an opportunity of observing the correspondence of other symptoms with the progress of the eruption. The result is, that in sixteen cases the symptoms became aggravated to a greater or less degree in proportion as the eruption advanced and became complete. The aggravation consisted of increased rapidity of the circulation, heat, and restlessness ; and at the more advanced periods of delirium also in some of the cases. A great increase of the eruption in some of those cases preceded the aggravation of the other symptoms for a day, in others, they appeared to be nearly simultaneous. Four of the twenty-two cases died, and were among the sixteen. In one case only of the whole number did improvement in the symptoms correspond with the full development of the eruption. In five there occurred little or no change in the symptoms, on the eruption becoming copious.

Seriousness of Cases compared with the state of the Eruption.—The importance of the state of the eruption as an indication of the seriousness of the fever in individual cases may be learnt from the amount of the mortality among those with much, compared with the mortality among those with scanty, eruption. Sixteen deaths happened among the cases in which the eruption was noted ; and in ninety of the whole number, the amount of the eruption was recorded as being copious or considerable, or scanty. In sixty-five cases, the eruption was copious or considerable, and in twenty-five, scanty. Thirteen of the deaths occurred among the former number, and three among the latter. Two of the three had extensive visceral disease of long standing; the one, a mason, of dissipated habits, had the lungs much disorganized by tubercles; the other, a female, had chronic bronchitis and emphysema of the lungs. Of the thirteen, none were

known to be such bad subjects for fever; seven of them had an eruption of the most copious kind; in six, it was considerable and general. Another method of ascertaining the relation which existed between the quantity of the eruption and the severity of the fever, is the comparing the average duration of the disease in the two categories of cases. Thus in fifty-five cases of abundant eruption in which the dates of the commencement of the illness and of the convalescence were ascertained with precision, it appears that the average duration of the intervening period was thirteen days and two-thirds. In the other class, many of the cases were so mild that reports were not taken so regularly as to enable me to ascertain exactly the time at which convalescence began in some of them. In eighteen cases the dates are sufficient, and show the average duration to have been eleven days and a half. There was extremely little difference between the sexes in these respects. By the commencement of convalescence, I mean the earliest considerable improvement in the state of the pulse, of the evacuations, of the tongue, of the temperature, and of the intelligence.

The convalescence of cases with the eruption in abundance was not so speedily completed as in the other cases. The only data I possess on this subject are the periods which elapsed between the commencement of convalescence and the time of dismission from the house. The latter, however, is not to be depended on as a criterion of completed recovery, since many circumstances on numerous occasions served to delay dismission totally unconnected with the state of convalescence. I shall not therefore enter at length into these details, but as a specimen of the difference in question, the following may be given. On taking, without selection, about twenty cases from each class, the average residence in the hospital of those which

had a scanty eruption appears to have been nineteen days, while that of the others reached twenty-five.

Relation between the Symptoms and the Decline of the Eruption.—It appears in a former paragraph, that of forty cases in which the first appearance of decline in the eruption is mentioned, in thirty-one it occurred on or before the twelfth day; and in order to determine how far the first decided indications of convalescence corresponded with the decline of the eruption, I propose to compare this with the averages ascertained in respect to the date of convalescence. The average date of commencing convalescence, taking all the cases, was the thirteenth day. The average date of commencing decline of the eruption in the forty cases was the eleventh. On inspecting the records of many of the cases, it appears, in accordance with these results, that convalescence was preceded for a day or two by a decline in the colour and abundance of the eruption. When cases were late of assuming the first appearance of convalescence, in them the eruption was prolonged beyond the average term; thus, in one case not decidedly convalescent before the twenty-first day, the eruption is reported as being fainter only on the seventeenth, still accompanied by hot skin, deafness, and a pulse beating a hundred and twenty in the minute. In another, with convalescence commencing on the seventeenth day, the eruption is reported as continuing abundant on the fourteenth; and in a third, convalescent on the eighteenth, the eruption became fainter only on the fifteenth. Nor did these circumstances occur in cases which presented the eruption first at a later period than usual, for in a case convalescent on the sixteenth, which had an abundant eruption on the fifth, it was as distinct as formerly on the thirteenth day. A considerable number of examples might be added; and it appeared a general rule, that when the fever was protracted (in-

dependently of the mere supervention and irritation of local disease) the eruption likewise exceeded its ordinary duration.

Petechiæ and Sudamina.—Purple petechiæ, the result of ecchymosis, were frequently present in the second week of the disease. In some cases they were very abundant, but in very few exceeded the number of typhoid *maculæ*, which commonly existed along with them. One *vibex* only was remarked in the whole number of cases. The petechiæ continued often distinct, though the eruption was declining. Sudamina were observed only in three instances, notwithstanding the frequent and careful inspection of the skin. The cases in which this eruption was noticed presented them on the tenth, eleventh, and thirteenth days.

7. *Ages.*—Among a hundred and nineteen females whose ages were ascertained, there were aged sixty, one case; fifty and below sixty, three cases; forty and below fifty, ten cases; thirty-five and below forty, seven cases; twenty-five and below thirty-five, thirty-three cases; twenty and below twenty-five, thirteen cases; ten and below twenty, forty-one cases; five and below ten, ten cases; and one case at four years old. Among the men, the ages were ascertained in forty-two cases: between fifty-five and fifty-eight, there were three cases; forty and below fifty, five cases; thirty-five and below forty, one case; twenty-five and below thirty-five, twelve cases; twenty, one case; ten and below twenty, fifteen cases; below ten, five cases.

Age in connexion with Mortality.—Thirteen females died, and in ten the ages were ascertained; of the other three, one appeared above forty; the other two between thirty-five and forty. Assuming these accounts to be correct, the following is the state of the deaths in connexion with the ages; one died aged sixty; two between forty and forty-five; three between thirty-five and forty; three

respectively at the ages of thirty-three, thirty, and twenty-nine; two at twenty-three; one at eighteen; one at fourteen. This statement gives a mortality above thirty-five of one in three and a half; between twenty-five and thirty-five, a mortality of one in eleven; and of one in sixteen and one-fourth below twenty-five. These proportions are only approximations to the truth, because of the hundred and thirty-two female patients that were treated before this account of the fever was begun, with the addition of sixteen that have been under treatment since, there are twenty-nine of whom the ages were not ascertained.

Among sixty-six male patients seven deaths occurred in the course of the fever; and two, after the proper symptoms of the fever had subsided, died in consequence of thoracic disease on the thirty-second and thirty-third days, without their ever having been so far convalescent as to have left their beds; while a third, who had recovered so far as to walk about the ward, and indeed was quite well on the twelfth day, died on the twenty-fourth of latent double pleurisy, and subarachnoid effusion. Omitting the last case, nine deaths occurred among the men; one at the age of fifty-eight, three at forty, one at thirty, four between twenty and twenty-five, inclusive.

8. *Dates, circumstances, &c., of the Deaths.*—Altogether, adding the sixteen cases alluded to above, a hundred and forty-eight female patients had fever, thirteen of whom died; wherefore, the deaths amount to one in nearly eleven and a-half. Two admitted in a very bad state died in about forty-eight hours after. Four cases died between the twelfth and eighteenth days with bronchitic symptoms predominant; three with predominance of cerebral disorder, much subsultus, stupor, and in one convulsions; two of these on the ninth and tenth days; the date of the other was not ascertained. One case died of *cynanche*

membranacea, extending into the larynx, on the sixteenth day. Five died between the eleventh and nineteenth, apparently of mere sinking and exhaustion, the most prominent symptom having been feebleness of the circulation, though the powers of the system generally were greatly enfeebled long prior to death. Nine of those fatal cases were admitted between the fourth and eighth days inclusive.

In five of the fatal cases among the men death was preceded for a considerable time by stupor, in two accompanied by hurried respiration. In two the respiration was chiefly disordered, one having had previously considerable bronchitis, the other without apparent bronchitis, extremely rapid respiratory acts, as many as eighty in a minute, for twenty-four hours before death, the pulse not becoming feeble till within a few hours of the close, and gangrenous spots occurring on the skin. The two cases that died so late as the thirty-second and thirty-third days had, the one gangrene of the lung around a tubercular mass, pleurisy, and bronchitis; the other much dilatation of the bronchial tubes, and pneumonia around them. The proportion of deaths among the men, including the two last cases, was one in seven and a third.

9. *Propagation, &c., of the Fever.*—Whether any examples of fever actually occurred without there having been previous intercourse with the sick, we had no means of ascertaining; but at least a fourth of the cases denied exposure to contagion, and ascribed their illness to cold; while about as many could not refer it to either contagion or cold. Of thirteen persons who were in attendance in various capacities on the cases described here, six had the fever previously, and four became affected with it for the first time, while officiating about the sick. About one-half of the patients traced their illness to intercourse with others affected with the disease. Cold weather had commonly

the effect of increasing the number of admissions, which declined again when the temperature was moderate. These fluctuations were noticed not merely on a general and large scale as on comparing the effects of summer and of winter, but even in the latter season occasional changes of weather, though not persisting above eight or ten days, had the effect I have mentioned.

10. *Treatment.*—The only details into which I shall enter on this subject are those which illustrate the effects of two opposite plans of treatment adopted at different periods of the epidemic. As constituting a general practice at every period, the moderate and steady use of purgatives, the occasional application of leeches and blisters, and the administration of gentle diaphoretic remedies, may be specified. The following details refer to the effects of wine and of blood-letting.

In the course of the first two months of last winter fifty-two female cases of fever were under treatment. During this period part of the practice pursued consisted of local remedies when pain existed, and of wine, as the disease advanced, when the pulse did not possess any considerable degree of firmness and size, even though not actually weak. The object in view having been, if possible, to prevent a further decline of the circulation, until the fever had attained its natural term of duration. Twenty-nine of the cases had wine in accordance with this view; twenty-two of these received wine first between the seventh and twelfth days inclusive, one only earlier. The average quantity given at first was four ounces and a half daily. This was subsequently increased in twelve cases to quantities varying from eight to twelve ounces; eight of these also appeared to require from three to six ounces of spirits daily. The average duration of the cases thus treated was, till the commencement of convalescence, fifteen days and a half. That of the apparently

milder cases which had no wine was less than eleven days. Six deaths occurred in the fifty-two cases, or, deducting a case, aged sixty, in a hopeless state at the time of admission, five deaths happened among those who were in no particularly unfavourable circumstances when admitted into the wards,—giving a proportion of one in ten. All the five were admitted before the eighth day of the fever, and, with the exception of one aged thirty-six, were between eighteen and thirty-three years old, and not more deficient in apparent robustness than the generality of females about the same time of life. So many deaths of persons seemingly favourable subjects for fever induced me, subsequently to the end of December, to adopt a contrary course of treatment. Between the end of December and the end of July last, ninety-six females were admitted. Of this number, thirty-six were bled from the arm. The average quantity from each patient was twenty ounces; one of them, from whom but a small quantity could be had from the arm, had subsequently sixty-three leeches applied to various parts. Eight cases were bled to thirty ounces and more, for the most part at two different periods on successive days. One of these lost forty-one ounces; one forty-two; one thirty-eight; two thirty-six; one thirty-three; eight between twenty and thirty.

The circumstances looked for in almost every case as indicative of the propriety of blood-letting were, that the fever should not have been in an advanced stage, the individual not of a delicate or previously enfeebled constitution, the pulse at least firm, whether small or full, and either particular local suffering, or general pains, restlessness, and flushing. In three instances some important particulars of this list were not attended to, and two of them were fatal. The one, though apparently robust, had recently recovered from an illness, and was reported to have been habitually in delicate health, the other was a drunkard,

and had been nursing for some months. These were the only deaths among the cases that were bled, giving a proportion of one in eighteen. Five of the thirty-six cases required wine at a late period of the disease, but only in one were stimulants administered freely; and, with the exception of two, they were bled to less than the average quantity, viz., fourteen ounces and under. Two of the cases that had wine were those that died,—the one on the ninth day with stupor and convulsions, the pulse not remarkably feeble; the other on the thirteenth, with hurried breathing and small soft pulse on and after the tenth. Of the ninety-six cases, twenty-four which were not bled, had wine from the ninth to the fifteenth day of the disease, two excepted, which had wine on the seventh. The average quantity begun with was three ounces, and in six cases only did it exceed five daily; three of these had above eight ounces.

Duration of the Cases.—The average duration of the cases that were bled and recovered, as ascertained in twenty-eight cases, was eleven days and two-thirds, up to the commencement of convalescence. The average of the twenty-four that had wine was fifteen days.

Deaths.—Of the ninety-six cases, seven died; or, subtracting cases that were admitted after all prospect of benefit from treatment was over, five died, giving a proportion of deaths, resulting from a cautious and sparing use of stimulants, and occasional blood-letting, of one in nineteen, or, including the two, one in fourteen nearly.

Dates of the Blood-letting.—Thirty were bled on or before the eighth day of the disease, two as late as the eleventh, one on the twelfth. The two fatal cases were bled, the one on the seventh day to twenty ounces—died on the thirteenth; the other on the sixth to fourteen ounces—died on the ninth.

Ages of those bled.—One was aged forty-nine; thirty-

five and below forty there were two cases; thirty and below thirty-five, four cases; twenty and below thirty, fourteen cases; fourteen and below twenty, twelve cases. The cases that died were aged twenty-three, and twenty-nine.

Appearance of the blood and state of Symptoms.—In ten cases the blood was natural in colour and the crassamentum firm; in two of these the venesection was practised after the eighth day, viz., on the eleventh and twelfth. Four of them had some cough with much pain of the chest, aggravated by full inspiration before the blood-letting; two had abdominal tenderness; four had chiefly severe headach, general distressing soreness, and restlessness. In five the pulse was above a hundred and twenty, small and firm; in the others between that and ninety, and of good strength. In all of these cases speedy relief to pain was the consequence of the loss of blood; in two, however, not till the evacuation was repeated once and twice respectively. In one case the favourable change was temporary, and death happened six days after.

In six cases the blood was distinctly sizy; in two cupped also. Five of them were bled between the fourth and the eighth days; one on the tenth. Before the evacuation, five had severe headach, and either much general soreness or tenderness of abdomen, or pain of the chest, with cough; one had much soreness, oppression, and pain of throat without headach. The pulse ranged from a hundred to a hundred and twenty-six, mostly small and tense; in one a hundred and twenty-six and full; in another, a hundred, and moderate. The effects were in all, after a first or second bleeding, much relief or total removal of the pains, and feelings of oppression; and in five the pulse had fallen in frequency on the following day from sixteen to twenty-six beats.

In four cases the crassamentum, in other respects

natural, presented a greenish or olive tint on the surface. These were bled between the fourth and eighth days, with permanent relief to the previous pains in the head and chest. In six cases the crassamentum, though natural in colour, had less firmness than that of healthy blood. Two of these were bled on the sixth day; one on the seventh; two on the ninth; one on the eleventh. Those bled on the two last dates had the crassamentum of the least consistence. With the exception of two, who were bled with much relief to twenty and twenty-seven ounces, those last cases did not bear venesection well. One died on the ninth day, and in the others, the disease extended much beyond the average of the cases that were bled, for it lasted on an average sixteen days, and one extended to the twenty-first.

Among the men only thirteen cases were bled; the average quantity was twenty-five ounces; the largest was forty-six ounces, in the course of four days, which was borne well, and succeeded by recovery. Ten of them were bled on the fourth, fifth, sixth, and seventh days; two on the ninth; and one on the twelfth, for the first time. Twenty-four ounces were the most taken at one time, and in patients of both sexes, the quantity at each venesection was regulated by the effects, either relief of the symptoms, or feebleness of the pulse. With the exception of one, aged forty-six, and of another, aged fourteen, the ages, as far as could be ascertained, were between eighteen and thirty. Two died, one aged twenty-five, the other thirty. The former, a robust man, was bled only to twelve ounces on the seventh day; and on the tenth, the pulse was moderate in strength, and beat a hundred and eight. Sudden cerebral symptoms came on on the eleventh, and after being furiously delirious for two days, he died comatose. The latter, bled between the sixth and ninth days to forty-two ounces, died on the fourteenth, having had very rapid respirations and delirium,

but not a feeble pulse, (beating at a hundred and twenty-four and a hundred and twenty-six,) for two days before his death. He had several gangrenous patches on the integuments of the chest, commencing on the twelfth day. The cases are too few, and the details too imperfect, to render a further analysis of them interesting.

The only other facts connected with the treatment, which are worthy of being mentioned, relate to the means which were used to subdue the bronchitis. When not attended with much expectoration, and when no other circumstances contraindicated the practice, blood-letting, leeches, tartar emetic, and blisters, were the ordinary remedies. In a considerable number of cases, however, the abundance of the secretion appeared to threaten danger, while these remedies were inadmissible, the blisters excepted. Under these circumstances, very great service was derived from the acetate of lead. It was given in doses varying from half a grain to two grains, several times a day, and usually in union with small quantities of the compound powder of ipecacuan, and one or two grains of squill. The effect in restraining the secretion was often apparent in a few hours. In several very severe cases of bronchitis with profuse secretion and rapid breathing, doses of tincture of cantharides, given alternately with the acetate of lead, appeared to hasten the reduction of the secretion. Some of these required a pretty liberal use of wine at the same time.—W. H.

APPEARANCES OBSERVED AFTER DEATH.

As superintendent of the pathological department in the Royal Infirmary of Edinburgh, I have had occasion to conduct the *post mortem* examination of a considerable number of fever patients, and I shall endeavour in the following part of this paper to give an analysis of the morbid appearances which were observed. I find on consult-

ing the Statistical Register of the Infirmary, that 2037 fever patients have been treated in that institution within the last fifteen months, and of these, 276, or one in $7\frac{105}{276}$ died. Of these 2037 patients, 962 were males, and 1075 were females. Of the males, 160 or one in $6\frac{2}{160}$ died; and of the females 116 or one in $9\frac{31}{116}$ died. From certain imperfections in the mode of keeping the Statistical Register, but which are now remedied, the above numbers are not to be considered as quite accurate, but they at least furnish an approximation to the truth. The number of fatal cases which I have personally examined within the period referred to, amount in all to forty-seven. Among these are not included six cases where the patients were seized with some other disease which proved fatal, during convalescence from fever. I may state, that more than forty-seven fatal cases of fever have been inspected in the Infirmary during the last fifteen months, as I have not included the cases which were inspected during three months when the disease was raging with great severity, as I was absent from indisposition.

Of these forty-seven fatal cases of fever, thirty-one were males, and sixteen only were females, so that the males form nearly two-thirds of the whole number. The average age of these forty-seven patients was $35\frac{28}{47}$ years. Of these, six were between fourteen and twenty years of age, twelve between twenty and thirty, eleven between thirty and forty, nine between forty and fifty, seven between fifty and sixty, and two between sixty and seventy. The average duration of the disease before death, as calculated from forty-three cases in which this was supposed to be ascertained with tolerable accuracy, was twelve and a half days. The earliest death occurred on the 6th, the latest on the 22d day of the disease.

The brain was examined in forty-three cases out of the forty-seven. Of these forty-three cases increased effusion

of serum within the cranium was observed in twenty-five, or in more than a half. The most common seat of this effusion was under the arachnoid. In five out of the twenty-five the quantity of serum effused was sufficient to elevate the arachnoid above the surface of the convolutions only at the most depending parts. In other five out of the twenty-five the effusion of the serum under the arachnoid was slight, but in one of these there was increased effusion into the cavity of the arachnoid; in a second, the lateral ventricles contained nearly three drachms of serum; in a third there was increased effusion into the cavity of the arachnoid, and a considerable quantity flowed from the sulci on slicing the brain; in a fourth, the lateral ventricles were evidently, though slightly, distended with serum; and in the fifth, seven drachms of serum were removed from the lateral ventricles. In fourteen cases out of the twenty-five, the quantity of serum effused was sufficient to elevate the arachnoid above the convolutions, not only at the posterior or depending parts of the surface of the brain, but also over the middle parts. In one case out of the twenty-five the arachnoid was not only elevated above the surface of the convolutions at the posterior and middle parts of the brain, but also over the anterior or most elevated parts, and the lateral ventricles were also slightly distended. Out of the fourteen cases in which the quantity of serum effused was sufficient to elevate the arachnoid at the posterior and middle parts of the surface of the brain, the lateral ventricles contained an increased effusion of serum in five; in one case this amounted only to between two and three drachms; in another case to between three and four drachms; in two cases to one ounce; and in one case to ten drachms. In one of these fourteen cases there was a dark-red spot in the left *corpus striatum*, of the size of a sixpence, apparently produced by great congestion of the blood-vessels, and there was also slight disease of the

large arteries of the brain. In those cases where the quantity of serum effused was sufficient to elevate the arachnoid above the surface of the convolutions at other parts besides those most depending, it also flowed out, and in many of these in considerable quantity, from the sulci in slicing the brain. The vessels of the brain in all the cases except in one, in which this organ was examined, were well filled with blood, and in many, a greater number than usual of red points presented themselves on the cut surfaces of the brain. In all except in one, the substance of the brain was of natural consistence; and in this case the softening was slight, extended throughout the whole brain, and occurred during hot weather.

With regard to the symptoms which were observed during life, in the twenty-five cases in which an increased effusion of serum was found within the cranium, and recorded in the journals kept by the physicians and their assistants, I find that those which may be supposed referable to the head are the following:—In the five cases in which the serum effused was sufficient to stretch the arachnoid above the surface of the convolutions only at the most depending parts, one is stated to have been confused; another was restless, and, according to the report of the nurse, had a convulsive fit the night preceding his death; a third had stupor with tracheal râles two days preceding his death, soon became comatose, and continued so up to the time of death; a fourth had headach on admission with muscular tremor. In the case in which there was no increased effusion under the arachnoid, but where there was increased effusion into the cavity of the arachnoid, the patient is stated to have been restless and delirious the third evening preceding his death, and on the morning of his death had *subsultus tendinum* and muscular twitches. In the case in which the lateral ventricles were slightly though evidently distended, without any increased effusion under the arach-

noid, the patient is reported to have been violently delirious, requiring coercion during the night previous to his death. He remained delirious next day, and died in the evening. In the case in which seven drachms of serum were effused into the lateral ventricles, without increased effusion under the arachnoid, the patient when admitted on the eighth day of the disease, and two days before death, complained of headach, and the pupils were contracted.

In the fourteen cases in which the quantity of serum effused under the arachnoid was sufficient to elevate that membrane above the surface of the convolutions at the posterior and middle parts, one had delirium at night, (this was the case in which between three and four drachms of fluid were effused into the lateral ventricles;) a second had much muttering delirium during the last two days he lived; a third talked incoherently, and had the face flushed the day before death; a fourth had on the day before and on the day of his death convulsive movements of head, neck, and shoulders, preceded by a girding headach; a fifth had no alarming symptom until the morning of death, (twelfth of the disease,) when he became completely insensible; a sixth had headach on admission, (seventh day of disease,) for which leeches were applied, and became delirious on the morning of death, (seventeenth;) in a seventh the head was pretty clear until the fifteenth day of the disease, which was the second day before death, when she became talkative and incoherent. The pupils were contracted, and the respirations were noisy on the morning of death. In an eighth the patient, three days before death, complained much of headach, and two days before death the pupils were contracted, extremities cold; pulse ninety-two, (formerly one hundred and eight;) countenance anxious, with moaning and delirium at night; in a ninth the patient was comatose, and the pupils were contracted the day before death; in a tenth, the patient was previously subject to fits of insanity,

and she was insensible for several days previous to her death; in an eleventh, the patient, three days before death, had much *subsultus tendinum*, and a tendency to stupor, with contracted pupils, (in this case one ounce of serum was removed from the lateral ventricles;) in a twelfth, (the case in which ten drachms of serum were effused into the lateral ventricles,) it is stated that he became oppressed and low, and had vomiting two nights previous to death; on the day previous to his death he attempted to get out of bed, and had constant tremor of lower jaw, restlessness, and moaning; he put out his tongue when desired, but gave no other evidence of intelligence; he became comatose at two o'clock next morning, and died at ten, A.M. In the case in which the serous effusion extended over the anterior part of the brain, the patient had much muscular tremor for two days before death, and was comatose for several hours before death. In the other cases in which increased effusion of serum was found within the cranium, they appear to have had only the confusion of thought so commonly observed in fever; and it is of importance to remark, that, in one of those cases in which one ounce of serum was effused into the lateral ventricles, and the arachnoid elevated above the surface of the convolutions at the posterior and middle parts of the brain, the disease ran its course without any marked local symptom.

We have now to inquire if, in the eighteen cases where no increased effusion of serum was found within the cranium, any symptoms presented themselves similar to those we have described as occurring in the cases in which an increased quantity of serum was effused, as it is only in this way that we can ascertain what relation there is between the appearances observed after death and the symptoms which manifested themselves during life. In one of these eighteen cases, it is stated that she had furious delirium on the second night previous to her death. In a second case, there was

much headach and muttering delirium for a few days previous to death; (this was the case in which the brain was unusually pale.) In a third, there was on admission, headach, suffusion of the eyes, and flushing of the face; he became comatose on the morning of his death; the face and extremities became livid, with feeble pulse, and he died ten hours after. In a fourth case, there was severe headach on admission, (fifth day of disease, and the fifth before death,) for which leeches were applied to the temples; on the seventh day of the disease, he was drowsy, and the pupils were contracted, and on the day of his death he became comatose. In a fifth case, the patient on admission was unable to give any account of himself; the pulse was quick, and of good strength; the eyes were suffused; the pupils were contracted, and he had slight subsultus; he remained insensible, and died on the third day after admission. In a sixth case, the pupils were contracted, and the patient gradually became more confused and feeble. In a seventh case, the patient was admitted in a drowsy state, the respirations were short and hurried, and she died next day. In an eighth case, the patient was delirious for six days after admission, but became calm three days before death. In a ninth case, there was no very unfavourable symptom until the night of the fifth day of his disease, when he became delirious; the pulse gradually sunk, and he died on the seventh day. In a tenth case, no urgent symptom had presented itself up to the night of the eighth day of the disease; she had remained tolerably quiet after an opiate until three o'clock in the morning, when she began to mutter indistinctly, and died in about five minutes. In an eleventh case, the patient had reached the seventh day of the disease without a dangerous symptom, when he suddenly fell back while sitting up in bed, and shortly after expired. In a twelfth case, the patient the evening before death had a convulsive fit, with movements of arms and

lower extremities, and distortion of features, which lasted for about ten minutes; she had two similar attacks within four hours; she became insensible after the first convulsive attack, and died next morning. In a thirteenth case, the patient when admitted, the day before death, had muscular tremors, the intellect was confused, and he answered questions imperfectly. In a fourteenth case, the patient was delirious, and attempted to escape out of a window two nights before death, and on the night preceding death he was very restless.

From this review of the *post mortem* appearances observed in the brain after death from fever, and the symptoms with which they were attended during life; and from the contrast we have instituted between those cases where an anormal quantity of serum was found within the cranium, and those cases where the usual quantity only was observed, we think we are justified in concluding, that they afford no distinct evidence that the serous effusion was in all cases, if in any, the cause of death. We have seen one case in which one ounce of serum was effused into the lateral ventricles, and yet nothing different from the usual confusion of thought was observed. We have also seen the cerebral derangement as strongly marked in those cases where no increased effusion of serum was found within the cranium after death, as in those where this was observed. Besides, it must be remembered, that it is not unusual to find increased serous effusion within the cranium in old people, or when the patient has been emaciated by previous disease, in quantities equal to what we have described as occurring so frequently in fever. And this last statement naturally leads us to inquire into the probable effects of age, and the duration of the disease, upon the amount of this serous effusion. I find that the average age of the cases in which an increased effusion of serum was found within the cranium, was $42\frac{21}{25}$ years; while the average age of those in which no

increased effusion was found within the cranium was $26\frac{16}{18}$ years. The average age of the four patients in which seven drachms or upwards of serum were found within the lateral ventricles was $57\frac{1}{4}$ years; the youngest being forty-eight, and the oldest sixty-eight years of age. With regard to the average duration of the disease in the two classes of cases, we do not find so striking a difference. The average duration of the disease in the cases in which an increased effusion of serum was found within the cranium (as calculated from twenty-four cases) was $12\frac{22}{24}$ days; while the average duration of those cases in which no increased effusion of serum was observed within the cranium was $11\frac{11}{18}$ days, or only a difference of nearly one day.

It has already been stated that, in most of the cases in which the brain was examined, we observed the blood-vessels well loaded with blood, as indicated by the numerous red points which presented themselves on the cut surfaces of the brain. In judging of the degree of vascular congestion in the brain, we ought to remember that part of the increased quantity of blood in the vessels of the brain may be merely apparent, and arise from the fluid state of the blood; for it is obvious that, if the blood remain fluid, the pressure of the knife used in slicing the brain will force the fluid blood through the open mouths of the vessels upon the cut surfaces.

In judging of the probable causes of the congested state of the blood-vessels of the brain, and of derangements of its functions, we must take into account the state of the respiratory organs; for it is apparent, that if there be any impediment to the passage of the blood through the lungs, this may influence materially the circulation within the cranium, even in those cases where the respiratory function is only secondarily affected through derangement of the central organs of the nervous system; for it is equally obvious that when from this cause there is any impediment

to the circulation through the lungs, it will react upon the central organs of the nervous system, and increase the primary derangement. And this leads us to examine the condition of the *lungs* in those cases in which the brain was examined. In the forty-three cases in which the brain was examined, the lungs were examined in thirty-nine. In the twenty-five cases in which an anormal quantity of serum was effused within the cranium, the lungs were examined in twenty-three; and in the eighteen cases in which no anormal quantity of serum was effused, the lungs were examined in sixteen. In the twenty-three cases in which more than the usual quantity of serum was effused within the cranium, the lungs, in one case, were scarcely engorged with blood, even at the depending parts; in three cases they were found simply engorged with blood at the depending parts; in one case the engorgement was combined with the effusion of frothy serum; in two cases, with frothy serum and the presence of old tubercles in both lungs; in one case with old pneumonia of right lung; in five cases with the effusion of frothy serum, and a greater or less quantity of mucus into the bronchial tubes; and in ten cases the lungs were congested at the posterior and middle parts with blood and frothy serum, and in some parts were so dense as not to crepitate when cut, and in three out of the ten cases, part of the dense portions when cut out sank in water, though they did not present the usual granular appearance of the second stage of pneumonia. Of the thirteen cases in which the lungs were examined, where there was no increased quantity of serum found within the cranium, in eight these organs were simply engorged at the posterior parts to a greater or less extent; in two cases this was combined with the effusion of a considerable quantity of frothy serum; in one case there was recent pneumonia of the upper part of one lung; in two cases the engorged state of the depend-

ing portions of the lungs and the effusion of frothy serum, were combined with an increased quantity of mucus in the bronchial tubes; and in three cases the posterior and middle parts of both lungs were gorged with blood and frothy serum, and some parts were so dense as not to crepitate when cut. In comparing the frequency of the lesions of the lungs in the cases in which an increased effusion of serum was found within the cranium, with those cases in which this was awanting, they will be found to preponderate in the former, particularly in that kind of lesion last-mentioned, and which it is to be remarked would be attended by great impediment to the free aëration of the blood, and, consequently, to its free circulation through the lungs.

It is probable, however, that the increased effusion of serum within the cranium, and the greater plenitude of the blood-vessels of the brain, occur too frequently to be fully accounted for by the age of the patient, the previous emaciation of the body, and the derangement of the respiratory function, and that it may, in a few cases, be owing entirely to, and in others much aided by causes connected with the nature of the disease itself, and by which that disturbance of the cerebral functions so generally observed in fever is induced. The facts which we have stated ought, however, to render us very cautious in attributing the phenomena observed in any individual case of fever, to increased plenitude of the blood-vessels, or to the effusion of serum.

The lungs were examined in four cases in which we were not permitted to inspect the brain. In one of these they were congested with blood at the depending parts, and a small portion of both lungs was œdematous; in a second case the larger bronchial tubes and a great number of the smaller, were full of puriform matter, and there was a small cluster of old tubercles in the apices of both lungs;

and in the two remaining cases there was a considerable quantity of frothy and puriform mucus in the bronchial tubes of both lungs. In all, then, the lungs were examined in forty-three cases. In ten of these there was increased effusion of mucus into the bronchial tubes; in thirteen cases the posterior and middle parts of both lungs were gorged with blood and frothy serum, and some portions were so dense as not to crepitate when cut, though they did not present any granular appearance; in one case a great part of one lung was dense from old pleuro-pneumony; in one case the upper and back part of the right lung was in a state of recent pneumonia, passing into the third stage; and in three cases there were old tubercles in the lungs. In fifteen cases out of the forty-three, the lungs were perfectly normal, with the exception of a greater or less degree of simple congestion of the most dependent parts. I would have hesitated in classing the case where acute pneumonia running on to the third stage was found, among the cases of fever, and would have been inclined to believe that it was a case of latent pneumonia, attended with typhoid symptoms, or that the pneumonia had occurred during the convalescence from fever, had I not been assured by Dr. Peebles, under whose charge the patient had been placed, that the usual febrile eruption on the skin was present, and that the pectoral symptoms came on before any decided signs of convalescence had presented themselves. When fever is prevalent, cases of latent pneumonia in an advanced stage, and attended by typhoid symptoms, are very readily mistaken for continued fever, unless the attention of the physician be particularly directed to the chest. I examined the bodies of three patients within a short time, who had been sent into the Infirmary as cases of continued fever, but, from the appearances observed on dissection, there could be little doubt that they were cases of latent pneumonia which had

gone on to the third stage, with very little of the usual pectoral symptoms. In the thirteen cases in which the posterior and middle parts of the lungs were loaded with blood and serum, and some portions were so dense as not to crepitate when cut, (resembling, as we have elsewhere stated,[1] the appearances observed after section of the vagi nerves,) these anormal appearances were in all probability principally dependent upon the disturbed respiration consequent upon derangement of the central organs of the nervous system, and generally occur a short time before death. This may also arise from the enfeebled action of the right side of the heart, for when the heart's action is feeble, the bronchiæ somewhat obstructed, and the blood more fluid than usual, the right side of the heart is unable to propel the blood through the lungs, it consequently goes on accumulating in the depending parts, and the same results follow as when the respiratory movements are deranged.

From the Journals I find that the following symptoms referable to the derangement of the respiratory organs were observed during life. One had cough before she was attacked by the fever, which continued during the disease, but with no great severity, but the breathing became much oppressed. In a second case the patient had cough and expectoration of long standing. He had been a stonemason, and had dark hard tubercles disseminated through his lungs. In a third case, the patient had oppression of breathing with mucous râle, and moaning, for six days before death. In a fourth case, the patient when admitted on the eighth day of the disease, and the seventh before death, had cough and expectoration. In a fifth case, the patient, on the seventh day of the disease, and the seventh previous to death, had cough and pain in the chest. In a

[1] Vide pp. 204, 205 of this volume.

sixth case, the patient, two days before death, had stupor with tracheal râles. In a seventh case, (the case in which the recent pneumonia was observed,) the respirations were very hurried for more than twenty-four hours before death, but no cough was remarked. In an eighth case, the patient on the twelfth day of the disease, and the seventh before death, had some oppression of the breathing, with mucous râles, and the respiration continued oppressed up to her death. In a ninth case, there was cough and expectoration, attended by dyspnœa. In a tenth case, the patient, on the second day of admission, and the twelfth day of the disease, was seized with cough, and catarrhal râles were heard in both sides of chest. On the seventh day of admission, and the seventeenth of the disease, the respirations were hurried, the face livid, with loud mucous râles in chest. On the tenth, she ceased to expectorate, and died on the twelfth day after admission, or the twenty-second day of the disease. This patient had been for some years liable to bronchitis on the slightest exposure to cold. It is perfectly obvious that these lesions of the lungs are not the cause of fever, and are not even essential to it; yet they frequently occur during its progress, complicate the disease, and render it more dangerous.

We next proceed to state the result of the examination of the Abdominal Organs. These were examined in forty-one cases out of the forty-seven. In these forty-one cases the elliptical patches of Peyer were apparent and distinctly defined in twenty-four, and in four of these the solitary glands at lower part of ileum were also distinctly visible. In six cases out of the forty-one, they were indistinctly defined and scarcely visible; and in eleven cases they could not be distinctly recognised by the naked eye. In all the cases in which the elliptical patches were distinctly visible, except in two, they were of a bluish colour, or rather of a greyish colour, dotted over with dark spots. In four cases

only were these patches distinctly elevated ; in two of them this elevation was to no great extent, and limited to a few patches. In two only were these elliptical patches very distinctly elevated, and presented any appearance of ulceration. The appearance of the elliptical patches, which we have described as most commonly observed,—viz. of a greyish colour with small dark dots on the surface, and without any distinct elevation, and no appearance of increased redness or ulceration,—is not peculiar to fever, but is not unfrequently observed in various other diseases, though not with the same frequency in other diseases as in fever. Some of the cases in which the elliptical patches and solitary glands were not visible had been actively purged during the progress of the disease by doses of calomel and jalap. On calculating the average duration of the disease in the cases in which the elliptical patches were visible, and comparing the result with a similar calculation of the duration of the disease in those cases in which the elliptical patches were not visible by the naked eye, I find that there was not any great difference in the average duration of the disease in the two classes of cases. In the cases in which the elliptical patches were visible, the average duration of the disease was $13\frac{23}{24}$ days ; and the average duration of the disease in these cases in which they were not visible was $11\frac{9}{11}$ days, or a difference of nearly two days. The average age of the two classes of cases was nearly the same. Out of the eleven cases in which the elliptical patches were not visible, two only were females. The mesenteric glands were distinctly enlarged in two cases only. In one of these they were at the same time softened, in the other they were nearly of their usual consistence. We have already stated that in two cases only out of forty-one in which the intestinal canal was minutely examined, were the elliptical patches of Peyer very distinctly elevated, and ulcerated. As the lesions of the

intestinal canal, observed in fever, have excited considerable interest and discussion, we shall describe these two cases more in detail. One of these was a male, aged twenty-five years. On admission, he stated that he had left the county of Sligo (Ireland) eleven days before, and that he felt rather unwell at that time. When admitted his skin was hot and dry; pulse frequent; countenance depressed; bowels constipated. On the evening of the second day of his admission he was very delirious, and made an attempt to escape through a window, and cut his face considerably. Pulse ninety-six; tongue dry; had a dose containing a drachm of sulphate of magnesia, one ounce of infusion of senna and one drachm of tincture of jalap, without any effect. Had much restlessness during the evening of the fourth day of his admission, and died next day. The bowels were not opened freely until shortly before death, when he had taken, besides the above-mentioned cathartics, two colocynth pills, half an ounce of tincture of senna, and had an assafœtida enema. He made no complaint of the abdomen. The body was inspected twenty-four hours after death. No increased effusion of serum was found within the cranium. The elliptical patches of Peyer were very distinctly elevated over an extent of twenty feet of the small intestines, and the solitary glands over an extent of five feet, both measurements commencing at the ileocæcal valve. These elliptical patches were of a grey colour, with a greater or less tinge of red at different parts, and at some places projected 3-24ths of an inch above the surrounding healthy mucous membrane. At the lower part of the ileum the elliptical patches were irregular on the surface, and presented several superficial and ill-defined depressions (ulcerations). About two feet above this they appeared somewhat flocculent on the surface, and were studded with numerous small rounded depressions. At the upper part of the ileum their surface was covered with a number of dark grey dots, without any

distinct depressions. On making a longitudinal section of these patches through the whole thickness of the intestinal tube, the textures of the peritoneal and muscular coats were unchanged, though the latter appeared somewhat thickened at the seat of some of the patches ; and that part of the cellular coat which lay next the muscular also presented its normal appearance. A mass of a greyish colour, with red lines indicating blood-vessels ramifying through it, adhered to the inner surface of the cellular coat, and projected into the cavity of the intestinal canal. On attempting to trace the mucous membrane over the surface of these morbid masses, this was found to be impossible ; and no distinct line of demarcation could be observed between the mucous surface and the morbid masses. It required some slight force to break up these enlarged patches with the nail, and they were softer on their inner surface than in the interior. The solitary glands were nearly of the size of split-peas, also of a greyish colour, and not ulcerated on the surface. The mesenteric glands were considerably enlarged,—the largest measuring $1\frac{2}{12}$ths of an inch in length, the same in breadth, and 6-12ths of an inch in thickness, the next largest 10-12ths of an inch in length, and 3-12ths in thickness. They were of a greyish colour, with small yellowish masses disseminated through them; and they could be broken down by a comparatively slight force.

The other case was a girl, aged fifteen years, who was admitted on the tenth day of the disease, and died on the twenty-second. She had no diarrhœa during her illness, but the bowels were easily moved by laxatives. Two days after admission she was seized with bronchitis, which was the most prominent local affection during the remainder of her disease. The elliptical patches of Peyer at the upper part of the ileum were defined, of a bluish colour, but without any distinct elevation. About a foot above the *caput cæcum*, one of the elliptical patches was distinctly elevated,

and of a red colour. A similar patch was placed immediately below this. About five inches above the termination of the ileum, another patch presented itself, more distinctly elevated than the two last described, of a greyish colour, and having two small superficial ulcers on one of its margins. Immediately below this was another patch still more elevated, also of a greyish colour, with several superficial ulcers on its surface. The lower part of the ileum was occupied by a large irregular patch, less elevated than the last described, partly of a reddish and partly of a grey colour, and presenting several irregular superficial ulcers on its surface. Several of the solitary glands at the lower part of ileum were also enlarged, and some of them were ulcerated on the apex. The mesenteric glands were enlarged, but were little, if at all, softer than usual.

On examining the Register of Dissections, kept by my predecessor, the late Dr. John Home, I find that, between 1833 and the beginning of 1837, the post mortem appearances observed in 101 cases of fever have been entered, in which the abdominal organs were examined. Of these 101 cases, the elliptical patches are described as being well defined or enlarged in twenty-nine. In seven of the twenty-nine a greater or less degree of ulceration of the patches was observed; and in two out of the seven perforation of the intestines had taken place.

It is an interesting and instructing fact, that, though lesion of the intestinal canal is comparatively rare in the fever of Edinburgh, at least for the last several years, the form of fever, as far at least as the post mortem appearances are concerned, described by Louis and Chomel, is to be found thirty miles from Edinburgh, at Anstruther, in Fifeshire. My friend, Mr. John Goodsir, junior, has shown me several preparations of this kind, which he has procured in the course of his practice there, and has furnished me with the following details:—

Within the last five years he has attended in Anstruther and its neighbourhood about one hundred cases of fever annually. It is there comparatively a mild disease, for of these only about sixteen died. He succeeded in procuring a post mortem examination in ten, and in every one of these, the elliptical patches of Peyer, and the solitary glands at the lower part of the ileum, were elevated and ulcerated, and in four perforation of the intestines had taken place. The symptoms attending this form of fever are—lassitude, pain of back and limbs at the commencement; afterwards the pulse ranges from ninety to one hundred and ten; there is ringing in the ears, but seldom delirium; a tendency to looseness, but seldom diarrhœa; generally heat in abdomen and groins, and uneasiness on pressure over the right iliac region, seldom amounting to pain; strong pulsation in iliac arteries; generally tympanitic swelling of abdomen; and more or less bronchitis. The greater number of fatal cases occurred in individuals from thirteen to twenty years of age. Mr. Goodsir has observed the morbid alterations of the elliptical patches of Peyer to pass through the following stages:—1. They are merely slightly elevated, exhibiting an undulatory surface; 2. A number of bluish dots appear on their upper surface; 3. These dots coalesce into a slough; 4. The slough assumes the form of a cake from one-fourth to one-eighth of an inch in thickness; 5. This cake begins to separate around the margin, so as to exhibit a subjacent ulcer; 6. The cake is separated; the last point of adhesion between the cake and subjacent parts being sometimes at the centre, at other times at one of the edges. The solitary glands undergo the following changes:—1. Slightly elevated; 2. Like a split-pea; 3. Rough appearance on the surface of the elevation; 4. Slight slough on the surface; 5. The central substance drops out, and leaves an ulcer with a hard base. In all these cases the mesenteric glands were enlarged and softened. In the four cases in which

perforation had occurred this was found to arise from the ulceration of one or more of the solitary glands. The disease was confined in general to the lower three feet of the ileum.

The only other anormal appearances in the intestines which we observed in the forty-one cases we have examined were the following:—In one case there were a few ecchymosed spots in the jejunum, and in upper part of ileum. In another case there was a morbid grey elevation adhering to the mucous membrane, about half an inch in breadth, and extending round a considerable part of the calibre of the upper part of the ileum. In a third there were some old cicatrices at the lower part of the ileum, and there were numerous circumscribed red spots on the mucous surface of ascending and transverse colon, without any decided change of the consistence of the membrane. In a fourth there was a number of small depressions, with surrounding redness, in the mucous membrane of the *caput cæcum*, ascending, transverse and descending colon. In a fifth there was a number of small depressions, without surrounding redness or thickening in *caput cæcum* and ascending colon. In a sixth there were numerous small red patches scattered over the inner surface of the ascending and transverse colon, with slight sponginess and elevation of the parts reddened.

Out of the six patients who died of other diseases while convalescing from fever, the abdominal organs were examined in four. One died on the thirty-first day after the commencement of the disease; another on the twenty-seventh day; a third on the twentieth day; and the fourth on the thirty-third day. In the three former there were no cicatrices of ulcers at the lower part of ileum; and in the last, the elliptical patches of Peyer were of a uniform dark-red colour throughout the whole tract of the ileum, were distinctly defined and elevated, irregular on the surface, and at one or two points presented traces of incipient ulceration.

In nine cases only out of the twenty-four in which the

elliptical patches were distinctly visible, were there any abdominal symptoms during life, and in some of those cases these certainly could not be referred to any affection of the elliptical patches of Peyer. In one of these the patient had retching and diarrhœa on the fifth day of the disease; this soon subsided, and he died on the eighteenth. In a second there was epigastric tenderness and tympanitis on the seventh day of the disease, and he died on the tenth. In a third the abdomen was distended and tender. In a fourth there was diarrhœa the day before death. In a fifth there was diarrhœa on the eighth day of the disease, (died on the seventeenth,) and the abdomen was slightly tender on pressure. A sixth had four stools without laxatives two days before death. A seventh had diarrhœa on the eighth day of the disease; this continued up to the time of death, which took place on the fifteenth. An eighth had a tendency to diarrhœa, the bowels being easily and repeatedly moved by laxatives. A ninth, (the case in which red patches in ascending and transverse colon, with slight sponginess and elevation of the parts reddened, were found,) had five stools without medicine the day before death. In the case in which there was diarrhœa for several days before death, there were numerous small ulcers in the large intestines.

In one case in which the elliptical patches of Peyer were not visible, the bowels are stated to have been rather loose, the stools watery and dark-coloured. In other two cases there was considerable abdominal tenderness without diarrhœa.

The following lesions of the *stomach* were observed in the forty-one cases in which the abdominal organs were examined. In three cases the mucous membrane was thickened and mammillated at the pyloric and middle portions. In a fourth case the mucous membrane of the pyloric extremity of the stomach was slightly mammillated, and it was softened at its splenic extremity. In a fifth case all the coats

of the stomach were corroded at the splenic extremity by the action of the gastric juice, and an aperture was found there capable of admitting the points of three fingers. In a sixth the mucous membrane was everywhere thickened, mammillated, and firmer than usual. In a seventh case the mucous membrane of the stomach presented numerous rounded superficial depressions, with defined margins without thickening or increased redness. In an eighth case, the mucous membrane along the larger curvature near the pyloric extremity was of a red colour, friable, covered with a thick layer of mucus, and presented several small rounded depressions surrounded by a bright-red margin. In a ninth and tenth case the gastric juice was found to have acted after death to a considerable extent upon the mucous coat of the stomach at its splenic extremity.

In those cases in which the mucous membrane of the stomach was thickened and mammillated, it was ascertained that the patients had laboured under dyspeptic symptoms for some time previous to the commencement of fever.

The *spleen* of all the organs in the body was found most frequently in an anormal state, being generally larger than usual, soft, and in some cases almost diffluent. In two cases the spleen was weighed, and in one it was eleven ounces one drachm, and in the other fourteen ounces. In some of the other cases it must have been heavier. In three cases, however, I find it described as presenting its most usual appearance and consistence, and in other two cases it was not increased in size, though perhaps it was softer than usual.

Out of the forty-one cases in which the abdominal organs were examined, the kidneys were found more or less affected with Bright's disease in six; and the *liver* was more or less altered from its normal state in five. In one case in which the liver was apparently quite healthy the patient had jaundice for two days before death. This had arisen

from an arrestment of the secretion of the bile, as all the ducts were quite pervious. These anormal changes of liver were of old standing.

In three cases out of forty-three the *heart* was in an anormal condition. In one case the inner surfaces of the pericardium were universally and firmly adherent; in another case the arch of the aorta was dilated, with calcareous deposit on its inner surface, and the left side of the heart was somewhat hypertrophied; and in a third case the free edges of the aortic semilunar valves were thickened, but sufficient for the performance of their functions. In one case there was recent inflammation of the *larynx*.

In all the cases the *blood* appeared to be in a fluid state, or nearly so, in the large veins, but in several a greater or less number of coagula, generally, however small and soft, were found in the right side of the heart. In two cases the blood was found in a grumous state, in the right side of the heart; in one case the right side of the heart contained some dark-coloured coagula; and in fifteen cases it contained dark-coloured intermixed with decolorized coagula. In two of these cases decolorized clots were also found on the left side of the heart. In several of those cases in which the blood is considered to have been fluid, a few small coagula were found adhering to the *chordæ tendineæ* and *columneæ carneæ* of the right ventricle.—J. R.

No. XIX.

OBSERVATIONS UPON THE STATISTICS AND PATHOLOGY OF THE CONTINUED FEVER OF EDINBURGH.

(EXTRACTED FROM THE LONDON AND EDINBURGH MEDICAL JOURNAL FOR AUGUST 1842.)

During my residence in the Edinburgh Royal Infirmary, I prepared, at very considerable expense of time and labour, the materials for the following statistical tables:—

The first table exhibits a great difference in the average mortality from fever in different months, though there was no decided change in the mode of treatment. In the months of September 1839 and 1840 the average mortality is only one in $21\frac{1}{6}$, or 4·72 per cent.; while in the months of December of the same years it was as high as 1-$5\frac{2}{37}$, or 19·78 per cent. Our total ignorance of the cause of such differences forcibly points out the small advances which we have hitherto made in our explanations of the source, origin, and nature, of continued fever. The third table illustrates the very great dissimilarity which exists in the average mortality at different ages. The necessity of keeping such facts as these in view, when judging of the average mortality under different modes of treatment, if we wish to avoid serious sources of fallacy, even in the same epidemic, are so very obvious, that we need not insist upon them.

TABLE I.

Number of Fever Patients admitted into the Infirmary each month, from 1st July 1839, to 1st October 1841.

Months.	Fever and Febriculæ.		Died.		Febriculæ.		Months.	Fever and Febriculæ.		Died.		Febriculæ.		Total Fever and Febriculæ for both Months.		Recovered.	Died.	Average Mortality.	Mortality per cent.	Number of Patients who took Fever in the Infirmary.			
	M.	F.	M.	F.	M.	F.		M.	F.	M.	F.	M.	F.		Male and Female.	Male and Female.	Male and Female.			M.	F.	Died. M.	Died. F.
1839.							1840.																
July,.........	17	24	2	2	1	1	July,.........	44	35	3	4	3	3		120	109	11	1-10$\frac{10}{11}$	9 16	...	1	...	...
August,......	16	23	1	3	1	...	August,......	27	35	3	9	...	1		101	85	16	1-16$\frac{5}{16}$	15 84	...	...	...	...
September,..	28	34	2	0	3	...	September,..	35	30	3	1	1	4		127	121	6	1-21$\frac{1}{6}$	4·72	...	1	...	...
October,.....	26	22	4	4	3	...	October,.....	37	27	5	3	3	1		112	96	16	1-7	14·28	5	1	2	...
November,...	24	42	3	4	1	4	November,...	35	42	5	3	...	4		143	128	15	1-9$\frac{8}{15}$	10·48	4	3	1	...
December,...	31	38	7	7	0	1	December,...	53	65	14	9	...	7		187	150	37	1-5$\frac{2}{37}$	19·78	1	3	...	1
1840.							1841.																
January,....	39	32	4	1	2	3	January,....	62	93	8	17	3	4		226	196	30	1-7$\frac{8}{15}$	13·27	2	1	...	...
February,....	34	34	7	2	8	3	February,...	67	79	6	14	5	11		214	185	29	1-7$\frac{1}{29}$	13·55	...	1	...	...
March,.......	27	48	2	3	6	8	March,.......	40	67	9	6	3	7		182	162	20	1-9$\frac{1}{10}$	10·98	2	2	1	1
April,........	30	37	6	6	1	2	April,.........	39	54	5	6	5	3		160	137	23	1-6$\frac{22}{23}$	14·57	1	1	...	...
May,.........	47	36	7	5	9	2	May,..........	39	63	7	10	1	3		185	156	29	1-6$\frac{1}{29}$	15·67	...	...	...	...
June,.........	41	39	7	4	1	6	June,.........	45	55	11	3	1	4		180	155	25	1-7$\frac{1}{5}$	13·88	2	...	2	...
Total for 2 years,	360	409	52	41	36	30		523	645	79	85	25	52		1937	1680	257	1-7$\frac{138}{257}$	13·26	17	14	6	2
July,.........	57	69	8	13																			
August,......	50	43	8	6																			
September,..	56	36	6	4																			

Those Patients who took Fever in the Infirmary are included in the numbers admitted monthly.

TABLE II.

Fever Patients from Leith and the country admitted within two years.

	Fever and Febriculæ.							Febriculæ.	
	Total Male and Female.	Total Male.	Cured.	Died.	Total Female.	Cured.	Died.	Male.	Female.
Leith, from 1st July 1839, to 1st July 1840, ...	36	24	20	4	12	10	2	3	2
Leith, from 1st July 1840, to 1st July 1841, ...	118	63	52	11	55	48	7	4	4
Total for two years, ...	154	87	72	15	67	58	9	7	6
The country from 1st July 1839 to 1st July 1840, ...	79	60	46	14	19	18	1	2	..
The country from 1st July 1840, to 1st July 1841, ...	131	96	89	7	35	31	4	..	2
Total for two years, ...	210	156	135	21	54	49	5	2	2

The patients sent to the Infirmary from the country were almost entirely from the labourers employed in forming railroads.

TABLE III.

Exhibiting the Fever Patients admitted between 1st July 1839 and 1st October 1841, arranged according to their ages, and the average mortality at different ages.

Years of Age.	Total Male and Female Fever and Febriculæ.	Male.						Female Fever and Febriculæ.	Female.				
		Male Fever and Febriculæ.	Cured.	Died.	Average Mortality.	Mortality per cent.	Number of Febriculæ.		Cured.	Died.	Average Mortality.	Mortality per cent.	Febriculæ.
1– 5	32	14	14		1–15	6·66	5	18	18		1–14	7·14	1
5–10	156	76	70	6				80	73	7			
10–15	255	124	122	2	1–62	1·61	13	131	126	5	1–26$\frac{1}{3}$	3·81	5
15–20	414	174	165	9	1–19$\frac{1}{3}$	5·17	20	240	231	9	1–26$\frac{2}{3}$	3·75	21
20–30	644	312	283	29	1–10$\frac{22}{29}$	9·29	15	332	303	29	1–11$\frac{13}{29}$	8·73	23
30–40	367	166	129	37	1–4$\frac{18}{37}$	22·28	10	201	164	37	1–5$\frac{16}{37}$	18·4	15
40–50	238	111	73	38	1–2$\frac{35}{38}$	34·23	2	127	92	35	1–3$\frac{22}{35}$	27·55	12
50–60	94	44	28	16	1–2$\frac{3}{4}$	36·33	2	50	35	17	1–2$\frac{16}{17}$	34·	2
60–75	48	25	9	16	1–1$\frac{9}{16}$	64·00		23	13	10	1–2$\frac{3}{10}$	43·47	3
	2248	1046	893	153	1–6$\frac{128}{153}$	14·62	67	1202	1053	149	1–8$\frac{12}{149}$	12·37	82

Sixty patients, (thirty-two males and twenty-eight females,) included in the above Table, were still in the hospital on the 1st October, but were discharged before the Table was made up.

As it is often impossible to say, in many cases, whether the patient is labouring under a mild form of continued fever, or under some temporary febrile excitement arising from other causes, these have been separated from the more marked cases of continued fever, and arranged under the head of Febriculæ.

Alleged previous duration of disease in fever patients before admission,				$7\frac{1}{2}$ days.
Ditto,	do.	febriculæ,	do.	5 ...
Average residence of fever patients in hospital,				26 ...
Ditto	febriculæ	do.		8 ...

FEVER AND FEBRICULÆ.

Alleged duration of disease before admission,	7 days.
Average residence in hospital,	24 ...
The days of admission and dismissal were both included in calculating the average residence in the hospital.	
Fever patients admitted from 4th February to 30th September inclusive, whose legal residence was in Edinburgh or Leith,	576
Fever patients admitted whose legal residence was elsewhere, though resident in Edinburgh,	94
Fever patients admitted from various parts of the country,	101
	771

In drawing any inferences regarding the probable duration of a disease, such as fever, from the length of time that patients remain in hospital, it ought to be kept in mind, that various circumstances, besides the state of the patient's strength, may at times influence his residence in hospital during convalescence. The more or less crowded state of the wards—the poverty and friendless condition of the patient—the possession of a tolerably comfortable home, &c., all influence the period of the dismissal of the patient. As these circumstances, however, to a certain extent, counteract each other, their being taken into account, compared with those already enumerated, is of less importance.

In another place[1] I gave an analysis of the post mortem

[1] Edinburgh Medical and Surgical Journal, No. 141.

examinations of forty-seven cases of continued fever, which I had made in the Royal Infirmary of Edinburgh, between April 1838 and September 1839. Between the latter date and 1st September 1841, or a space of nearly two years, I have had occasion to inspect one hundred additional fatal cases of that disease in the same institution. I shall now endeavour to give an analysis of these from the details entered in the register of dissections kept by me in the Infirmary. Among these hundred cases are not included two, who were seized with and died of pneumonia while recovering from fever. I shall not enter upon an analysis of the symptoms which these cases presented during the course of the malady, as it would not furnish us with a correct view of the character of this epidemic. I would willingly have endeavoured to have given an analysis of the symptoms, both of the favourable and unfavourable cases, had I the necessary documents in my possession; but the want of such information is the less to be regretted, as I believe the character of the epidemic to be essentially the same as when Dr. Henderson drew up his excellent numerical analysis of the most important symptoms observed in nearly two hundred cases placed under his care.[1] Of these hundred cases, forty-four were females and fifty-six males. In the following Table they have been arranged according to their ages :—

[1] Report on the Epidemic Fever of Edinburgh, &c. &c., printed in this volume.

	Males.	Females.	Total.
From 5 to 20 years, ...	5	7	12
„ 20 to 30 „	12	9	21
„ 30 to 40 „	15	10	25
„ 40 to 50 „	12	10	22
„ 50 to 60 „	8	5	13
„ 60 to 70 „	4	3	7
	56	44	100

	Years.
The average age of the female patients, ...	$34\frac{3}{4}$
„ „ male ditto, ...	$37\frac{3}{4}$
„ „ both male and female, . . .	$36\frac{1}{3}$

	Average Duration of Disease before Admission.	Residence in Hospital.	Total Duration of Disease.
	Days.	Days.	Days.
Males, . .	$7\frac{1}{48}$	$5\frac{8}{51}$	$12\frac{10}{47}$
Females, . .	$6\frac{24}{47}$	$6\frac{6}{43}$	$13\frac{8}{41}$

Total duration of disease in both males and females before death, $12\frac{48}{89}$, or somewhat more than $12\frac{1}{2}$ days.

The earliest death took place on the fourth day, and the latest on the twenty-third day.

Of the 56 males, the brain was examined in . .	47
Of the 44 females, the brain was examined in . .	35
	82

Head.—In indicating the quantity of serum in the sub-arachnoidean cellular tissue, I have classified the cases under four divisions. In the first, I have placed all those in which the serum was either only sufficient to moisten the parts, or too small in quantity to cause the appearances characterizing the other three classes. In the second division, I have arranged those cases in which the sulci on the surface were wider than what we generally find in the brain of a young individual, and were occupied by serum, but without any very distinct elevation of the arachnoid above the surface of the convolutions. On slicing down the brain in some of these cases, a very considerable amount of serum flowed from the sulci. In the third and fourth divisions, the cases are arranged according to the extent to which the arachnoid was raised above the surface

of the convolutions. We have much more precise and accurate methods of ascertaining the quantity of serum present in the lateral ventricles. In every instance the lateral ventricles were carefully opened, and the fluid, if present in any appreciable quantity, was withdrawn by means of a pipette, and accurately measured in a graduated glass measure.

MALES.

	Numbers.	Average Age.	Youngest Years.	Oldest Years.
Small quantity of serum under the arachnoid, ...	27	$30\frac{19}{27}$	7	56
Sulci more or less wide, and full of serum, ...	12	$48\frac{1}{6}$	35	62
Arachnoid elevated at depending parts,	7	$52\frac{6}{7}$	44	67
Ditto ditto at depending and middle parts, ...	1	66	—	—
	47			
Serum in lateral ventricles less than ʒss	22	33	7	62
,, ,, between ʒss and ʒi	5	$44\frac{2}{5}$	27	57
,, ,, ʒi and ʒiss	2	39	35	43
,, ,, ʒiss and ʒii	5	$43\frac{4}{5}$	25	54
,, ,, ʒii and ʒiii	8	$44\frac{1}{4}$	17	67
,, ,, ʒiii and ʒiv	4	$40\frac{1}{2}$	25	49
,, ,, ʒv and ℥i	1	66	—	—
	47			

In seven among the most aged of these cases in which there was less than ʒss of serum in the lateral ventricles, a considerable quantity was found between the sulci. In two of the cases where the serum in the lateral ventricles was between ʒss and ʒi, the arachnoid was elevated above the surface of the convolutions at the depending parts. In four, where the serum was between ʒii and ʒiii, the arachnoid was elevated above the surface of the convolutions at the depending parts; and in one of these, ℥i, and in another ʒii of the same fluid were collected in the cavity of the arachnoid, while the heads were kept in an elevated position.

FEMALES.

	Number.	Average Age.	Youngest Years.	Oldest Years.
Small quantity of serum under arachnoid,	20	$27\frac{10}{19}$	12	44
Sulci more or less wide, and full of serum,	12	$46\frac{1}{4}$	34	60
Arachnoid elevated at depending parts,	3	$60\frac{2}{3}$	53	65
	35			
Serum in lateral ventricles less than ʒss	15	$26\frac{1}{14}$	12	40
„ „ between ʒss and ʒi	3	$27\frac{1}{3}$	15	44
„ „ „ ʒi „ ʒiss	3	$44\frac{2}{3}$	35	59
„ „ „ ʒiss „ ʒii	4	$43\frac{3}{4}$	37	50
„ „ „ ʒiii „ ʒiv	2	59	58	60
„ „ „ ʒiv „ ʒv	5	$54\frac{3}{5}$	45	65
„ „ „ ℥i „ ℥iss	3	38	34	40
	35			

In three of the above females in whose lateral ventricles between ʒiv and ʒv of serum were found, the quantity of that fluid beneath the arachnoid was sufficient to elevate it considerably above the surface of the convolutions; and in a fourth, ʒii were collected from the cavity of the arachnoid.

In the following Table the two preceding ones are thrown together.

TOTAL MALES AND FEMALES.

	Number.	Average Age.
Small quantity of serum under arachnoid, . .	47	$29\frac{17}{47}$
Sulci more or less separated, and full of serum, . .	24	$47\frac{5}{24}$
Arachnoid elevated at depending parts, . . .	10	$55\frac{1}{5}$
Ditto do. at middle and depending parts, .	1	66
	82	
Serum in lateral ventricles less than ʒss	37	$30\frac{11}{36}$
„ „ between ʒss and ʒi	8	38
„ „ „ ʒi „ ʒiss	5	$42\frac{2}{5}$
„ „ „ ʒiss „ ʒii	9	$43\frac{7}{9}$
„ „ „ ʒii „ ʒiii	8	$44\frac{1}{4}$
„ „ „ ʒiii „ ʒiv	6	$46\frac{5}{6}$
„ „ „ ʒiv „ ʒv	5	$53\frac{2}{3}$
„ „ „ ʒv „ ℥iss	4	$44\frac{3}{4}$
	82	

Of the eighty-two brains examined, thirty-four are described as presenting a considerable number of red points on the cut surfaces, while in three, the substance of the brain is stated to have been pale. The condition of the other brains, in regard to the quantity of blood which presented itself on the cut surfaces of the organ, was that which is most usually found in death from other diseases. In one patient, (a female aged sixty-four,) a portion of the substance of the anterior part of the right corpus striatum, about the size of a pea, was of a dark red colour, from the quantity of blood which it contained. The coats of the internal carotids were much thickened at several parts from a yellowish deposit. The basilar artery was less diseased. There was also serum under the arachnoid, elevating the membrane at the depending parts, and a considerable quantity of the same fluid flowed from the sulci on slicing the brain : ʒv of serum were also withdrawn from the lateral ventricle. The substance of the brain was of normal consistence. In one male, aged fifty-six, in whose lateral ventricles ʒvi of clear serum were found, there was distinct softening of the inner surface of the ventricles, without any change of colour, and apparently arising from inbibition of the serum. There was an appreciable difference in the consistence of the brain in different cases ; but we believe that this, as far as the disease was concerned, was an accidental circumstance, depending upon the age of the individual, the length of time that intervened between death and the inspection, the state of the weather, &c. We feel satisfied that similar differences in the degree of the consistence of the brain will be as frequently, or nearly as frequently, observed in death from other diseases, where we have no reason to believe that the brain is affected. In my former communication on this subject already referred to, I entered into some details in giving an account of the

inspection of the brains of forty-three persons who died of fever, with the view of ascertaining if the symptoms observed during life could be elucidated, or explained, by the appearances observed in that organ after death. I endeavoured to show that there was no necessary relation between the extent of the cerebral symptoms during life, and the quantity of serous fluid, or the quantity of blood in the vessels, observed after death. By referring to the history of the cases, we found that very little serous fluid was present within the crania of patients, who, during the course of the disease, had convulsive attacks with insensibility, had much delirium, or who died comatose; while, on the other hand, some of those, within whose crania the largest quantity of serum was found, only presented the usual confusion of thought attending fever. On contrasting the symptoms observed during life, in the whole 125 cases of fever in which I have examined the brain, with the appearances observed after death, I find that the following statements are equally applicable to the whole of the cases as to a part of them :—" We have seen the cerebral derangements as strongly marked in these cases where no increased quantity of serum was found within the cranium after death, as in these where this was observed. Besides, it must be remembered, that it is not unusual to find an amount of serous fluid within the cranium in old people, or where the patient has been emaciated by previous disease, in quantities equal to what we have described as occurring so frequently in fever." It has also been stated, that in many of the cases " in which the brain was examined, we observed the blood-vessels well loaded with blood, as indicated by the numerous red points which presented themselves on the cut surfaces of the brain. In judging of the degree of vascularity of the brain, we ought to remember, that part of the increased quantity of blood may be merely apparent, and arise from the fluid

state of the blood; for it is obvious, that if the blood remain fluid, the pressure of the knife in slicing the brain will force the fluid blood through the open mouths of the vessels upon the cut surfaces." In some of these cases, where the head symptoms were most violent and sudden, the quantity of blood found in the vessels of the brain after death was certainly not above the normal quantity. A girl, aged twelve, was seized on the twelfth day of fever, with convulsions, insensibility, and foaming at the mouth. Shortly after these symptoms had abated, she was seized with another convulsive fit, during which she died. Two days before death she complained much of frontal headach, for which she was bled to ℥x. On examination of the body after death, we found that the quantity of serum within the cranium was only sufficient to moisten the surfaces of the brain, and that that organ itself was rather pale, but everywhere of normal consistence. In the earliest case of death, (on the fourth day,) the patient, a female, aged sixteen and a half years, was admitted on the third day of the disease, and the symptoms were then comparatively mild. When admitted, she had headach and great pain in the loins; face flushed; pulse 100, of good strength; skin hot and dry; bowels costive; tongue coated with a yellowish fur; and she had frequent rigors on the two first days of her illness. She was ordered ʒvi olei ricini, and a sinapism to the loins. On the day after admission the pain of the back was much relieved; a number of large and florid petechiæ had appeared on the chest and back; the other symptoms were as on admission. In the evening, between five and six o'clock, she appeared to be drowsy; and about half-past six, the nurse remarked that her breathing was loud, and endeavoured to rouse her, but in vain. When I saw her at eight o'clock, she was lying on her back in a comatose state; her face was livid, and her breathing noisy; the respirations were eighteen in the minute, the-

racic, and very imperfectly performed; the petechiæ were livid; the eyes bloodshot, and the pupils much contracted. The pulse was then of tolerable strength, and not increased in frequency; but it gradually became more feeble as the respiration became less perfect, and she died at half-past eight o'clock, P.M. On examining the body after death, very little serum was found within the cranium, and the surfaces of the brain were simply bedewed by it. Every part of the brain appeared normal in colour and consistence, and no great number of red points presented themselves on the cut surfaces. The lungs were moderately congested with blood, and there was a small quantity of frothy serum in the bronchial tubes. The blood in the heart and large vessels was quite fluid; there were some bright red petechial spots below the pleuræ, on the posterior surfaces of both lungs, and a few bright red small petechiæ on the anterior surface of the right ventricle of the heart, and several irregular dark patches at the inner surface of the left ventricle, caused by the effusion of small quantities of blood below the endocardium; old adhesions existed between the anterior surface of the liver and the inner surface of the anterior wall of the abdomen; the liver was larger than usual, firm, friable, and granular, but not much changed from the natural colour, except that small masses of a yellowish colour were scattered through it; the kidneys were healthy, but around the pelvis and infundibula of the right, a quantity of dark coagulated blood was effused into the cellular tissue; there were several petechial spots under the mucous surface of the intestines, more especially about the lower part of the ileum and the caput cœcum; the elliptical patches were not elevated, but some of those at the lower part of the ileum were densely covered with petechial spots. It is probable that the complication of the purpura hemorrhagica may have been the cause of the early and rapid death in this case.

Even in those cases in which the greatest quantity of serum was present within the cranium, we had no reason to believe that effusion had taken place rapidly, or exercised any injurious pressure upon the substance of the brain, for in none was the dura mater stretched tensely over its surface, the convolutions flattened, or the sulci pressed together. There can be no doubt that the amount of serous fluid within the cranium is much influenced by the age and previous condition of the patient; and our impression is, that its amount in fever cases is not greater than in death from other diseases in which the brain is not supposed to be implicated; but as we intend, in an early number of THE MONTHLY JOURNAL, to give the result of extensive observations collected on this subject, we shall not at present enter upon it further.[1] On looking at the previous tables, it will be perceived that the average age of the three females, who had the largest quantity of serum in the lateral ventricles, was below that of others in whose brains a smaller quantity was found. The history of these cases, and a consideration of the whole post mortem appearances, are sufficient to show, that even in these, the presence of the serum could not be the cause of death. One of these was a female, aged thirty-nine, who stated that she had been ill for five days previous to admission. When admitted, she complained of headach; had some cough; the skin was hot and dry, with faint spots upon it; and the bowels were confined. The bronchitic symptoms increased, and small mucous râles were heard posteriorly on both sides. She died on the fourteenth day of the disease, and became comatose before death. A small quantity of serum was found under the arachnoid, and there was no increased vascularity of the surface of the brain. The substance of

[1] [In the Monthly Journal for April 1843, we have given tables showing the quantity of serum we procured from the lateral ventricles after death from various diseases.]

the brain was of good consistence, and the usual number of red points presented themselves on the cut surfaces: there were ℥x of pure serum in the lateral ventricles. The septum lucidum was much stretched, very thin, not softened, and was reticulated at the posterior part. The inner surface of the ventricles was not softened, but felt a little firmer than the rest of the brain; it had a more opaque appearance than usual, but was quite smooth. There was no flattening of the convolutions on the surface of the brain; the arteries of the brain were healthy. Now we feel satisfied that the post mortem appearances indicate that the effusion of the serum had been slow and gradual, and had taken place, or at least was going on, at the commencement of the fever, and was in all probability dependent upon a chronic inflammation of the inner surface of the ventricles, totally unconnected with the attack of continued fever.

Another of these cases was a female, aged thirty-four, a widow, who had, previous to the commencement of her illness, suffered many privations. When admitted under my care, she was in the eighth day of fever. The pulse was 120, and soft; the skin was warm, and covered with much eruption; some nausea; little headach; the eyes were suffused; and the bowels slow. She answered questions readily. Symptoms of convalescence showed themselves on the ninth day after admission, that is, on the seventeenth day of the disease, but she appeared to make very little progress, and on the twenty-fourth day of the fever, was much weaker, and vomited some bilious matter. She lay in a drowsy state, but could be readily roused. The eruption on the skin was completely gone. From this time she gradually became weaker, without any local symptom, and died on the twenty-sixth day of the disease, in a state of great emaciation. On examination of the brain after death, more serum than usual was found under the

arachnoid, and the sulci were somewhat separated from each other: there were ʒix of serum in the lateral ventricles. The brain in every other respect appeared normal. In a third case, the female was forty years of age. When admitted, she had febrile symptoms of no great severity, and was convalescent, when she was seized with rigors, headach, and increased frequency of pulse. The pulse continued frequent; she had delirium at night, *subsultus tendinum*, and at last a tendency to stupor. The breathing was oppressed, with some mucous râles in the chest. The tongue was dry; and the bowels had a tendency to be loose. She died on the twelfth day after her relapse. On examination after death, some serous fluid was found under the arachnoid, but not sufficient to elevate that membrane above the surface of the convolutions; there was ℥i of clear serum in the lateral ventricles. No softening of the inner surface of the ventricles was seen, and every part of the substance of the brain appeared to be of normal colour and consistence.

Before closing these details regarding the condition of the brain, we shall shortly state some *accidental* anormal appearances which we observed in that organ after death from fever. In a boy aged fourteen, admitted in a state of insensibility about the fifth day of the disease, the inner surfaces of the arachnoid adhered *firmly* over the anterior lobes of the brain, and the cineritious substance was there firmer, and adhered more strongly to the pia mater. It was stated by his relatives that he had received a blow on the head when a young boy, and had after this been subject to fits, but that he had none for four years previous to his admission. It was also stated, that he had become insensible on the third day of the disease. An Irish labourer, aged twenty-four, was admitted in a state of delirium, and, as far as could be ascertained, about the seventh day of the disease. He had been delirious for two

days before admission; and it was necessary to tie him down in the cart in which he was brought from a part of the country twelve miles distant. He died next morning. On removing the dura mater, some adhesions were found between the inner surfaces of the arachnoid, over the upper part of the left anterior lobe of the brain, and on attempting to break them through, a small rounded body, about the size of a pea, and of an almost calcareous consistence, was dragged out from a depression in the surface of the brain in which it rested, bringing a small part of the substance of the brain with it. There was no increased redness nor softening of the brain around this morbid deposit. In a male patient, aged sixty-six, there was circumscribed and superficial yellow softening of the cortical structure of the brain around the olfactory bulbs. The arteries of the brain were considerably dilated, but their coats were scarcely thickened, and a calcareous deposit was observed in one place only. In two aged males there was considerable disease of the large arteries within the cranium.

Chest.—The *lungs* were examined in eighty-eight cases. From the result of these, and of the forty-three cases which we have already analyzed, we feel satisfied that the posterior and middle parts of the lungs are more frequently found loaded with blood and frothy serum, than in death from other diseases in which the lungs are not primarily affected. We have found recent pneumonia more frequently in the present cases than in those we formerly analyzed. We may here take the opportunity of stating, that when any local disease was found, if the data would at all justify us in attributing the symptoms to the local affection, we struck the case from the list of continued fever. It was not without serious and careful consideration that we retained among those of continued fever the cases in which recent pneumonia was present. We have arranged the cases in which the lungs were examined, as follows:—

	No. of Cases.
Moderately congested with blood at depending parts, .	21
Considerably do. do. .	19
One lung congested considerably, the other moderately, .	2
Much congested, ...	17
So dense at depending parts as not to crepitate when cut,	8
Recent pneumonia, ...	11
Recent bronchitis to a considerable extent, . .	4
Recent pleurisy and pericarditis, ...	1
Portion of lung dense from late pneumonia, . .	3
Late pleurisy, ...	1
Considerable tubercular deposit, ...	1
	88

In five of these eighty-eight cases, not included among the cases of recent bronchitis and recent pneumonia, there was a quantity of mucus present in the bronchial tubes sufficient to interfere with the function of respiration ; and in other three there was considerable emphysema of the lungs, with some mucus in the bronchial tubes. Of the eleven cases of recent pneumonia, three were males, and eight females. In one of these, a considerable portion of the right lung was in the condition of grey hepatization, and there was a thin layer of puriform matter on the inner surface of the left pleura. The patient, a male aged sixty, had low muttering delirium, dark and dry tongue, and an eruption similar to that observed in cases of continued fever on the skin, while the thoracic symptoms did not bear any proportion to the general symptoms. It is quite possible that this case, notwithstanding its strict resemblance to the typhus fever, may yet have been a case of pneumonia attended with typhoid symptoms. In another case, a female aged fifty-eight, the lungs were dark coloured along their posterior edges, and did not crepitate when cut. The cut surfaces yielded, when pressed, much frothy serum with some puriform matter—the latter apparently from the bronchial tubes. A small abscess, about the size of an almond, surrounded by a portion of the lung in the second stage of pneumonia, was found in the upper

and posterior part of the lower lobe of the right lung. Portions of the posterior parts of both lungs were quite dense and granular, but friable, and containing much fluid—passing into the second stage of pneumonia. In one, to which we shall again have occasion to refer, the pneumonia was produced by the extension of the inflammation of the inner surface of the bronchial tubes. In the other eight cases, which occurred between the 16th December 1840, and the end of January 1841, or within a period of about six weeks, the appearances observed in the lungs had a general resemblance. There was a greater or less condensation of the posterior parts of the lungs, so that they did not crepitate when cut. The appearance of the cut surfaces differed from the ordinary characters of the second or third stages of acute pneumonia, in being nearly smooth, in being more compressible, or in yielding more when pressed between the fingers, and in being much less easily lacerated. Some portions were, however, less compressible, and presented a more granular appearance, especially when torn.

In none of these cases was there any pleuritic effusion. In two, a small quantity of puriform matter appearing to flow from the air cells, could be pressed from the cut surfaces. In some cases, as it is difficult to draw a line of demarcation between great condensation of the posterior part of the lungs, depending upon physical causes, and condensation arising from inflammatory action, it will be necessary to call in the aid of the microscope. The case included under *late pleurisy*, was a female, aged eighteen, who had pain of the right side of the chest for some days before she was seized with symptoms of continued fever. On examination of the body after death, a thin layer of lymph, dry, and of tolerable consistence, adhered to a part of the inner surface of the right pleura. The frequent congestion of the posterior and middle portions of the

lungs with blood and frothy serum, may perhaps be chiefly dependent upon disturbed respiratory movements, upon the enfeebled state of the right side of the heart, and upon the altered condition of the blood. The respiratory movements are very generally increased considerably in frequency, for some time before death, but at the same time they are short, and often appear to be imperfectly performed. If the action of the heart were becoming much enfeebled, the bronchial tubes somewhat obstructed by mucus, and the blood more fluid than usual, the right side of the heart would no longer be able to propel the blood through the lungs, and it would consequently go on accumulating in the depending parts, and the same results follow, as when the respiratory movements are deranged. The bronchial glands presented the same appearances as in death from other diseases.

Heart.—There was slight recent pericarditis in one of those cases of typhoid fever, in which perforation of the ileum took place. The other anormal appearances found in this organ were chronic, and though totally unconnected with the origin and nature of the fever, they would, by their presence, favour, if they did not occasion the fatal termination. In four cases there was sufficient organic disease of the left side of the heart to interfere with the perfect performance of the functions of the organ.

Abdominal organs.—These were examined in ninety-one cases. In the first cases examined, I recorded the particular condition of the elliptical patches and solitary glands; in the subsequent cases, I contented myself, when they were perfectly normal, by stating that they were neither reddened nor elevated. By the expression, *not visible to the naked eye*, I mean to express the fact, that the position of the elliptical patches could not be recognised by looking on the inner surface of the intestine, and that it was necessary to hold up the opened intestine between the

eye and the light before we could distinguish their outlines. The condition of the elliptical patches and solitary glands is exhibited in the following table:—

	Male.	Female.	Total.
Not visible to the naked eye,	3	3	6
Scarcely visible, . .	10	7	17
Distinct, but not defined, .	1	3	4
Defined, ...	5	3	8
Neither reddened nor elevated,	21	23	44
Elevated, but not ulcerated, .	2	4	6
Elevated and ulcerated, .	5	1	6
	47	44	91

In one of those cases included among the *elevated but not ulcerated*, one elliptical patch only was slightly elevated without any increased redness, and there was no enlargement of the mesenteric glands. In other two, the elliptical patches at the lower part of the ileum were elevated without increased redness, and presented on the surface the *barbe rasée* appearance, and in one of the two, some of the mesenteric glands were enlarged from the effects of former disease, for they were either hard or contained cretaceous matter. In another—a girl aged eight years—the elliptical patches in the lower six inches of the ileum were obviously elevated above the surrounding mucous membrane, were somewhat irregular on the surface, but not ulcerated; were of a greyish colour throughout, and their vertical section showed no yellowish or lardacious looking deposit in their interior. The mesenteric glands were slightly enlarged, but not decidedly softened. In a fifth, the elevation of the elliptical patches was apparently dependent upon acute inflammation of the mucous membrane of the ileum. In this case, there were also extensive colonitis, and we shall again have occasion to refer to it. In the sixth—an Irish labourer, aged twenty, and not resident in Edinburgh—the mucous membrane of the lower part of the ileum, for five and a half feet above the ileo-cœcal valve, was of a deep red colour, and the elliptical patches and the solitary

glands were evidently elevated and projected about one-twelfth of an inch beyond the surrounding mucous membrane. Numerous whitish spots were observed on the surface of the elliptical patches, and one on each of several of the solitary glands, but there were no distinct appearances of ulceration. The outer surface of the ileum, at the parts corresponding to the lower elliptical patches, was of a dark colour, from the quantity of blood which the vessels there contained. On making a vertical section of these patches, the morbid part was of a uniform red colour, and could be readily broken down by the nail of the finger. The mesenteric glands were considerably enlarged and much softer than usual.

In six cases out of ninety-one, the elliptical patches were elevated and ulcerated, and in five of these, the solitary glands were in the same condition. In all the six, the mesenteric glands were enlarged, and more or less softened. Of these six cases, perforation of the lower part of the ileum, producing rapid and fatal peritonitis, occurred in two. These six cases of ulceration of the elliptical patches may be divided into two distinct classes, into those in which the ulceration begins on the inner surface and gradually extends downwards, the edges of the ulcer do not present a sloughy appearance, and the interior of the enlarged patches contains no lardacious-looking deposit; and into those in which the ulceration commences by the detachment of sloughs formed in the interior of the enlarged glands, by the breaking up of a lardacious looking deposit, upon which the increased size of the elliptical patches and solitary glands depends. To the first of these divisions one only of these six cases belongs. This patient was a female, who died about the sixteenth day of the disease. Two irregular ulcers, each about an inch in circumference, were found in the lower part of the ileum, extending down to the muscular coat, and placed in the position of the

elliptical patches. A little above the uppermost of these ulcers, one of the elliptical patches was elevated, very vascular, and had commenced to ulcerate on the surface. On making a vertical section of this patch, its cut edges did not present any lardacious appearance. The mesenteric glands were very slightly enlarged. The five cases belonging to the second division were males, all labourers on the Glasgow railroad, who had been for a short time previous to their illness located in Linlithgow or its neighbourhood, about seventeen miles west from Edinburgh, and four of them were natives of Ireland. Their average age was twenty-five years; the youngest was eighteen, and the oldest thirty-five years. The most characteristic case of this description was the following:—When admitted, he could give no very distinct history of his illness, and the symptoms of continued fever were complicated with severe bronchitis. He had no diarrhœa during life. *Sectio cadaveris.*—Chest. The inner surfaces of both pleuræ were firmly adherent at the posterior parts. The lower and posterior parts of both lungs felt pretty dense, and this was more marked in the left than in the right lung. On making a section of the dense portion of the lung, a considerable quantity of serum could be readily pressed out, and the cut surfaces presented a mottled appearance, arising from a number of black spots scattered through the tissue of the lungs. These black spots we found on examination to be dense and firm, (portions of pneumonic lung,) following the course of the smaller bronchial tubes. The smaller bronchial tubes distributed in these dense portions of lung were dark-coloured, and very considerably dilated. The tissue of the lungs, in the intervals of these dense portions, contained a considerable quantity of blood and serum, but was spongy. The lower and anterior part of the upper lobe of the left lung presented two large bullæ, and the anterior margins of both lungs were light coloured and of

a doughy feel. The heart was normal. The blood was principally fluid, with some dark coloured soft clots in the right side of the heart.—Abdomen. The mesenteric glands, opposite the caput cœcum and lower part of the ileum, were considerably enlarged and softened. The largest of them was about the size of a large hazel-nut, and they were of a greyish colour tinged with red. Within the intestines, diseased elliptical patches and solitary glands extended over the four and a half lower feet of the ileum, but did not include all the elliptical patches placed in that portion of the ileum. At the termination of the ileum, there was a large irregular elliptical patch, four and a half inches in length, and one and a quarter inches in breadth at the upper part, while at the lower part it extended round the whole circumference of the intestine. It was very irregular on the surface, consisting of deep excavations with narrow ridges between them, projecting considerably above the level of the surrounding healthy mucous membrane. The height of the ridges above the bottom of the excavations was three and a half lines, and their edges, as well as those of the excavations, were sloughy and deeply tinged with bile. Scattered over the healthy mucous membrane at the sides of this diseased patch, were several small rounded and prominent elevations, of nearly the size of split peas. Some of these were smooth on the surface and entire, while others were beginning to break up in the centre. The excavations in the large patch in some places did not extend through the muscular coat of the intestines, while in others they had penetrated to the peritoneal. The only other elliptical patch diseased, was situated one foot and a half from the lower part of the ileum, and was about two inches in length. It presented the same appearance as the larger patch already described, except that the excavations were less deep, and the intervening ridges broader, and more sloughy. There were several rounded, and very

distinctly elevated bodies (solitary glands) scattered over the surface of the mucous membrane, between these two diseased patches. These isolated and rounded masses were at least three times larger than those placed around the large diseased patch. Some of them were completely excavated in the centre, each forming a round narrow ridge, surrounding a deep depression ; others had a slough in the centre, while others were entire and smooth on the surface. The mucous membrane above the smaller diseased patch, up to the point where it became quite healthy, presented an appearance similar to that between the two patches. The two uppermost elevations were less distinctly defined on the edges, and less elevated than those lower down, and were quite entire. On cutting through some of those elevations, which had not begun to break up, the cut surfaces were found to be firm, and of a lardacious appearance. Though, on the whole, the disease was more advanced in the lower part of the ileum than higher up, this was certainly not uniform, for some of the elevations were broken up in the form of sloughs, while others placed below them, or nearer the termination of the ileum, were still entire. The mucous membrane in the splenic end of the stomach was very thin and soft. The spleen was rather soft, and weighed ℥ix; the other abdominal viscera were normal. We were not permitted to examine the brain.

In another case, the elliptical patches and solitary glands in the lower part of the ileum, presented appearances very similar to those which we have described in the previous case; and in addition to this, there were found, in the ascending and transverse colon, numerous small, rounded, isolated, and elevated bodies, the largest equal in size to small peas, very similar in appearance to the diseased solitary glands in the small intestines, and they were also undergoing the same changes. In a third case, the disease of the glands or mucous crypts was confined to the lower

three feet of ileum. In a fourth case, one in which perforation of the ileum took place about three-fourths of an inch above the caput cœcum, some of the higher patches were more diseased than others placed lower down. In this case there were also recent pleurisy and pericarditis. In the fifth case, the perforation of the ileum was about two and a half inches above the caput cœcum.

In neither of the two patients who died of pneumonia coming on during convalescence from fever, were any traces of ulceration of the intestines to be observed. Conjoining the present cases with the forty-one which I previously analyzed, I have altogether examined the intestines in 132 cases of fever, not including six cases where the patients died of other diseases when convalescing from fever. Of these 132 cases, ulceration of the elliptical patches, or solitary glands, was found in eight, or one in sixteen and one-fourth. From the description which I have given of the character of these diseased mucous crypts in my former communication and in this, in five only, or one in twenty-six and two-fifths, do the morbid changes correspond with those which are considered characteristic of the typhoid fever of Paris. In all of those five cases, the patients were not resident within several miles of Edinburgh when they were seized with the disease. In two also of those cases where the ulceration of the elliptical patches commenced on the surface, and presented exactly the same appearances as we occasionally find in this tissue, independently of the presence of continued fever, the patients did not belong to Edinburgh, were Irish labourers in search of work, and had not been in Edinburgh before the commencement of their illness. During the whole three years and a half that I conducted the post mortem examinations in the Edinburgh Infirmary, in no single case did I observe, in any individual who had been seized with fever while residing in Edinburgh, anything resembling the changes described as

occurring in the lower part of the ileum in the typhoid fever of Paris. It is worthy of observation, that in other fatal cases of fever sent from Linlithgow and other places along the line of the railroad, no alteration upon the mucous crypts at the lower part of the ileum could be detected. Whether or not the typhoid and typhus fever be identical or different diseases, we shall not venture at present to give an opinion: but if it should turn out that they are specifically the same disease, it would prove an interesting subject of inquiry to endeavour to ascertain why the typhoid form of the fever should, for several years past, never be found in Edinburgh, while it existed at Linlithgow, Anstruther, and other places in Fifeshire.

In five cases there was extensive acute inflammation of the mucous membrane of the intestines. In one of these, a male, aged fifty, the mucous membrane throughout the lower five and a half feet of the ileum was much congested with blood. The elliptical patches were visible, but were neither elevated nor ulcerated. The whole mucous membrane of the large intestines was of a deep red colour; and in many parts of the ascending colon there were several dark coloured patches, caused by the effusion of blood into the submucous cellular tissue. In the sigmoid flexure and rectum, the inner surface of the mucous membrane was roughened, apparently from the effusion of small nodules of lymph. The mesenteric glands were slightly enlarged, but were otherwise normal. He died on the eleventh day of the disease. He had no abdominal distention or diarrhœa; but on the night preceding his death, he passed repeatedly, *per anum*, a considerable quantity of blood. In another case, that of a male, aged thirty-nine, the mucous membrane of the ileum, throughout an extent of three feet, measuring from the caput cœcum, was of a deep red colour, somewhat roughened, especially in the region of the elliptical patches, and flakes of a greyish adhesive substance,

(lymph, or very adhesive mucus,) were scattered over this portion of the mucous surface of the intestine, and were most abundant over the elliptical patches. The elliptical patches were very slightly elevated, and were not ulcerated. The ascending colon, the transverse and upper part of the descending colon, were of a dark colour when viewed externally; and when slit open, the mucous membrane was found thickened, spongy, almost black, from the quantity of dark blood which it contained; and there were several ecchymosed spots on its inner surface. The greater part of the caput cœcum, and a small part of the commencement of the ascending colon, were nearly normal. Some discharge of blood, *per anum*, also occurred in this case, the evening previous to his death. In a third case, the inner surface of the ileum, about five feet above the ileo-cœcal valve, was of a bright red colour, with numerous small elevations thickly set together, of a deeper red than the surrounding surface, and were formed by slight projections of the mucous surface. This condition of the mucous surface extended upwards for three and a half feet, with some small intervals, in which the mucous membrane presented its normal appearance. In a fourth case, the whole mucous surface of the large intestines, and of the inferior three-fourths of the ileum, was in a state of intense inflammation. In the fifth case, the mucous surface of the ascending and transverse colon was in a state of acute inflammation, and there was also Bright's disease of the kidneys. We need not dwell upon other anormal appearances observed in the intestinal canal, as we believe that they had no influence upon the course of the disease. We shall merely state, that in one, the mucous membrane of the intestine was considerably softer than usual, but there was no change of colour, and no increase in its thickness. In another case, numerous dark-coloured spots, which were round, small, and not elevated, (enlarged follicles,) were observed on the inner surface of

the commencement of the large intestines. Numerous scattered, small, firm, and grey granules, of the size of millet seeds, were also adhering to the inner surface of the lower part of the ileum. In seven of the ninety-one cases, there were distinct traces of chronic inflammation of the stomach; and we had evidence that some of these had suffered from dyspepsia before being attacked by fever. In eight of the ninety-one cases, the kidneys were more or less affected with Bright's disease; and in another, one of the kidneys was completely destroyed, and consisted only of a fibrous sac, filled with a substance not unlike coarse plaster of Paris. The other kidney was, however, quite healthy, nearly twice its usual size, and weighed more than nine ounces.

Spleen.—I have weighed the spleen in a considerable number of cases of death from fever; but as there is no organ so liable to variations in size within the standard of health, I shall defer any remarks which I may have on this subject, until I have had time to contrast the average weight with that in individuals dying of other diseases; and in the mean time merely remark, that though, in the greater number of cases, it appeared larger and softer than usual, yet in several it was undoubtedly neither.

Condition of the Blood.—In sixty-one cases the condition of the blood was specially noticed. In twenty-eight cases it was fluid in the large vessels, and in the cavities of the heart. In nine cases, some small dark and soft clots were found in the right side of the heart, but the blood was generally fluid. In twenty-two cases, dark and decolorized soft clots were intermingled with the fluid blood in the heart. In two cases there were many decolorized clots in the cavities of the heart.

[Dr. Peacock has published, in the Monthly Journal of Medical Science for 1843, p. 413, a Statistical and Patho-

logical Report of the Cases of Fever treated in the Edinburgh Infirmary during the year 1842, in which he gives an analysis of the appearances observed in thirty-one cases examined after death; and this report agrees closely with that drawn up by me. Dr. Bennett stated, in April 1846, (*vide* Monthly Journal of Medical Science for June 1846,) that during the three years he had superintended the inspections after death, as Pathologist to the Infirmary, he had only seen two examples of dothinenterites, or that kind of fever which has been called typhoid. In the winter of 1846-7, cases of typhoid fever became frequent; and Dr. Bennett (same Journal for February 1847, p. 624) had occasion, during that period, to examine upwards of twenty fatal cases of fever, in which the follicular lesion of the intestines was more or less marked. I have been informed that after this irruption of typhoid fever had lasted a few months, it again disappeared.

I have been pleased to find that these reports on the appearances observed in the bodies of patients who died of typhus fever have been considered of value by some of those who have had lately occasion to investigate this subject. Among others, Professor Bartlett, of the University of Maryland, in his work on Typhus and Typhoid Fever, mentions the first of these reports on the post mortem examinations of the bodies of fever patients, in the following flattering terms:—"These examinations are reported and analyzed, and compared with the symptoms, in a spirit the most philosophical, and with a completeness as rarely met with as it is worthy of the highest praise. They constitute a very valuable addition to our knowledge of the lesions in this disease."]

No. XXI.

CASE OF OBLITERATION OF THE VENA CAVA SUPERIOR AT ITS ENTRANCE INTO THE HEART.

(FROM EDINBURGH MEDICAL AND SURGICAL JOURNAL, JANUARY 1835.)

During the course of last month (December 1833,) the *vena cava superior* was observed to be obliterated at its entrance into the heart, in one of the subjects in Dr. Knox's Practical Dissecting-Rooms. This obliteration was about two inches in extent, and formed, in the usual situation of the vein, a rounded cord of a cartilaginous feel, firmly connected to the surrounding parts, particularly to the anterior surface of the right *bronchus*, at the division of the *trachea*, by firm cellular tissue. Around it, particularly at the lower part, were several calcareous deposits, apparently connected with the bronchial glands. On the inner surface of the auricle, the entrance of the vein was only marked by a slight depression, and the auricle at this part formed a kind of digital pouch.

From the advanced state of the dissection when this circumstance was discovered, it was difficult to trace the channels through which the blood must have passed to the *vena cava ascendens*; but after a good deal of careful examination, some of these, and perhaps the greater part, were made out. The large veins at the root of the neck united in the usual way to form the *cava superior*, and this vein became suddenly impervious at the part already mentioned.

On examining the *vena azygos*, it was found to be at least twice its usual size, and became suddenly impervious where it joined the obliterated *cava*. The right intercostal veins, particularly the superior, were much enlarged, and a vein of the size of a common quill passed between the *cava* immediately before its obliteration and the upper part of the *vena azygos*. The *vena azygos* was joined at the usual place, viz., about the seventh dorsal vertebra, by what is called the *semi vena azygos*, which was more than twice its usual size. The state of the superior intercostal veins of the left side could not be ascertained, as they had been nearly all destroyed: but, to judge of their size from some remnants of them, they must also have been considerably enlarged. On blowing air down the *azygos* and *semi-azygos*, they were observed to retain their large size, as low as the *diaphragm*, and to form several flexuosities. The air passed freely along the enlarged intercostals and superior lumbar veins, distending all the lumbar veins, and filling the *vena cava ascendens*. The lumbar veins were also considerably enlarged. A vein of at least the size of a writing quill, formed by a branch from each of the *azygos* veins, passed through the left side of the aortic opening, and joined the *cava inferior*, near its junction with the renal veins.

The internal mammary veins had been destroyed, but the epigastric veins were of their natural size, so that there could not be any reflux of blood in that direction.

All the systematic venous blood of the body, except that of the coronary vein, being poured into the right auricle by the *vena cava inferior*, that vein was also considerably enlarged. At its entrance into the heart, it nearly admitted the passage of three fingers. The *azygos* veins generally communicate with the renal veins; but whether this communication existed, or was enlarged, could not be ascertained, as the kidneys had been removed.

The condition of the vessels, as here described, was fully

examined by Dr. Knox and Mr. Fergusson, who assisted in the dissection.

The exact manner in which the blood passed from the large vessels at the lower part of the neck to the enlarged intercostals which entered the *azygos* veins, could not be examined, as the dissection was too far advanced; but it is probable that a considerable part of it flowed through the intercostal veins, which empty themselves into the *venæ innominatæ*, and then passed through the anastomosing branches of these and the intercostals which enter the *azygos* veins. Part of it might also have flowed along the internal mammary veins, passed through the anastomosing branches of these with the intercostals and superior lumbars, and thus reached the *azygos* veins. The thoracic veins in the *axilla* might also have allowed a reflux through them, which, from their anastomoses with the intercostals, would also reach the *azygos*. The sinuses and veins within the spinal canal might also have assisted in this, by their numerous anastomoses.

Though the *azygos*, intercostals, and lumbar veins were all enlarged, yet it was difficult to conceive how the whole of the blood of the *cava superior* could pass with the necessary facility into the *cava inferior*, particularly as it was a retrograde motion.

That there could be any other channel than those already mentioned through which the blood could pass between the two *cavæ*, it would be difficult to conceive.

The appearance of the parts around the impervious portion of the *cava* furnished no perfect clue to any supposed cause of its obliteration. It is, however, probable that the calcareous deposits around the vein were the remains of some enlarged bronchial gland, which, by their pressure upon the vein, produced its obliteration.

This subject was a female about forty years of age, and died in the City Charity Work-House, under the care of

Dr. Smith, who has kindly furnished the following particulars of her case :—

" A. B. was admitted into the Charity Work-House in July last, labouring under symptoms of *hydrothorax*. There was general *anasarca*, great difficulty of breathing, livid countenance, inability of lying down, scantiness of urine, pulse frequent, small, and irregular.

" She was relieved of these symptoms by the use of cream of tartar and other diuretics.

" Finding herself better, she left the house in September free from œdema, but shortly after her symptoms relapsed, and she was again admitted in October. The same remedies were had recourse to, but without producing any relief. She died rather suddenly, under an aggravation of all the symptoms on the 30th November, having had neither delirium nor coma, though she was somewhat deaf.

" *Inspection.*—The right cavity of the *thorax* was full of serum, and the *pleura costalis* was much thickened. On the left side there were strong adhesions between the *pleura pulmonalis* and *costalis*, and the latter membrane was in many places fully three lines in thickness. The kidneys exhibited strong marks of Bright's disease. This woman had led a very irregular life."

I may also mention, as somewhat analogous to this case, that last winter, while dissecting the sinuses of the *dura mater* I met with an obliteration of the superior longitudinal sinus about its middle. I ascertained that in this case also there had been no cerebral derangement present during the last illness of the patient.

In contrast with this case, where the blood of the two *cavæ* was poured into the auricle by the inferior *cava* alone, we may mention another which lately occurred in the same rooms,[1]

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[1] This subject was dissected by my friend Dr. P. S. K. Newbigging.

where the greater part of the blood of the two *cavæ* entered the auricle by the *cava superior*.

The two primitive iliacs in this case, instead of joining to form the *cava inferior* in the usual place, continued to ascend separately, the one on the right, and the other on the left of the *aorta*, until they were joined by the renal veins. They then united a little to the left, and behind the *aorta*, to form the *cava inferior*. This vessel passed through the aortic opening, getting to the right of the artery, and took the usual position and course of the *vena azygos*, and, like it, joined the *cava superior*. In this subject the depression on the inferior surface of the liver to the right of the *lobulus Spigelii*, for the *cava inferior* was awanting; and passing through the opening in the *diaphragm* for the *cava inferior*, we had a much smaller vessel formed by the junction of the hepatic veins, which took the place of the *vena cava inferior*, and, like it, opened into the auricle.

In this last case, the irregularity in the venous distribution evidently depended upon original formation, and is interesting as such;[1] while the two former apparently depended upon a change produced by disease on the original formation, and possess considerable interest both in a physiological and pathological point of view.

I may mention that the whole three subjects were females; the two first were about forty years of age, and the last about sixty-five.

[References to the other cases of obstruction of the superior cava on record are given by Professor William Thomson in the same Number of the Edinburgh Medical and Surgical Journal in which this case was published, and by Dr. Peacock in the London Medico-Chirurgical Transactions, vol. xxviii., p. 14. 1845.]

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[1] According to Meckel, the veins, particularly the internal ones, are seldom found to vary from their usual course, and in that respect differ considerably from the arteries.

No. XXII.

CONTRIBUTIONS TO FORENSIC MEDICINE.

(FROM MONTHLY JOURNAL OF MEDICAL SCIENCE FOR 1841, AND EDINBURGH MEDICAL AND SURGICAL JOURNAL, NO. 142.)

CASES WHERE A CONSIDERABLE QUANTITY OF BLOOD WAS FOUND EFFUSED INTO THE CELLULAR TISSUE WITHOUT EXTERNAL INJURY.

Circumstances might occur where it would be of importance for a medical jurist to be aware, that considerable quantities of blood may be effused into the cellular tissue, and among the muscles, during certain diseases, and there becoming coagulated, present the appearances of an injury inflicted during life, or very shortly after death. We can easily conceive how cases analogous to those about to be related may lead the medical jurist to very erroneous conclusions, if called in to examine the body after death under suspicion of some foul play having been practised upon the deceased. We do not remember of having met with similar cases in the works on medical jurisprudence which we have had occasion to consult.

Case I.—A woman, aged twenty-six, died of pneumonia eleven days after her admission into the Infirmary, and the body was examined about forty-eight hours after death. As the weather was cold no signs of putrefaction had manifested themselves. The features were full, and there was

much fat deposited below the integuments of the chest and abdomen. In cutting through the anterior wall of the abdomen, there was a considerable quantity of dark-coloured and coagulated blood found effused into the sheath of the recti muscles, and extending from the pubis up to the umbilicus. A quantity of coagulated blood had also been effused along the lower margin of the right false ribs, and was situated in the cellular tissue below the external oblique muscle. The integuments covering those parts were of their natural colour. The appearances, in fact, resembled the effects of blows produced by a blunt instrument impinging upon a considerable extent of the surface of the body, but there was not the slightest reason to believe that any such injury had been inflicted.

Case II.—A man, aged thirty-nine, died of Bright's disease of the kidneys, four weeks after his admission to the hospital. It was not until four days after death that permission was procured to examine the body, and it was beginning to pass rapidly into decomposition. On cutting through the abdominal walls a considerable quantity of dark-coloured coagulated blood was found effused into the sheath of the recti muscles around the umbilicus, without any external appearances indicating its presence.

Case III.—A man, aged forty, died of continued fever, three days after his admission. On inspecting the body twenty-four hours after death, a considerable quantity of dark-coloured coagulated blood was found in the sheath of the recti muscles around the umbilicus. The integuments covering this part presented their natural appearance.

Case IV.—A woman, aged twenty-nine, died of confluent small-pox on the eighth day after the appearance of the eruption, and the fifth after her admission into the hospital. On opening the body, the cellular tissue below one of the pectoralis major muscles was found extensively infiltrated with blood.

These four cases are the only ones of their kind which I have remarked in about 500 inspections which I have made in the Royal Infirmary; but as a limited part only of the surface of the body is cut into in making these inspections, it is quite possible that similar appearances might have been observed in other situations in these and in other cases, had other parts of the body been examined.[1]

The following case is also worthy of notice:—

Case V.—A male patient under the charge of Mr. Fergusson, now Professor of Surgery in King's College, London, died of erysipelas of the left leg, and of the perineum; and on uncovering the body, about twenty-four hours after death, for the purpose of making a *post mortem* examination, it was observed that the skin over the anterior and middle part of the leg affected with the erysipelas had assumed a dark purple colour, resembling so strongly the appearance of ecchymosed blood consequent upon injury, that all who were present confessed, that, if they had not been aware of the previous history of the case, they would have had little hesitation in attributing these appearances to some injury received during life. The anatomical examination of the limb, apart from all knowledge of the history, would, however, have disclosed to us, that this ecchymosed appearance had not been caused by external injury, for, on cutting into it, no blood had been effused into the cellular tissue, but the skin was increased in thickness, and its vessels loaded with dark blood.

[1] [Since the preceding remarks were published, I found, in inspecting the body of a lady advanced in life, and who had been confined entirely to bed for several weeks before her death, a large quantity of dark-coloured coagulated blood below the pectoralis major muscle of one side, and a smaller quantity under the corresponding muscle of the opposite side. There was no reason to believe that this was the result of any injury during life. Dr. Bennett informs me that during the period he has conducted the post mortem examinations at the Royal Infirmary, he has met with effusions of blood into the sheath of the recti abdominales muscles in two or three instances.]

The following case is interesting, as showing the dissimilar appearances presented by blood effused into different parts of the body, from injuries received thirty-one days before death :—

Case VI.—An old woman, aged seventy, threw herself over a window during a fit of nervous excitement induced by smoking tobacco, and was placed under Dr. Robertson's charge. She had received a severe lacerated wound of the scalp, the scull was extensively denuded, and there was a simple fracture of the sternum and tibia. She died thirty-one days after the injury, gradually exhausted by loss of blood, by the irritation and discharge from the wound of the scalp, and from sloughs forming over the sacrum.

Sectio cadaveris, twenty-six hours after death.—The denuded surface of the cranium was covered with granulations, except at two places near the vertex, where the bone was quite bare, and at one part rough. On removing the skull-cap, a fracture was found running obliquely upwards and backwards in the left parietal bone, beginning near the anterior and inferior angle. A layer of coagulated blood, about two lines in thickness, and about two inches in breadth, was found in the left temporal region, lying between the dura mater and the inner surface of the cranium. In one place it was of a brownish hue, but the greater part of it was still pretty dark-coloured. On the right side of the head, there was a similar effusion of coagulated blood, but it was situated inside the dura mater, and on the surface of the arachnoid covering of the brain—or, in other words, within the cavity of the arachnoid—and was placed more posteriorly than that on the left side. This last mass of coagulated blood was everywhere of a chocolate colour, showing that the process of absorption was much more advanced than on the left side.

A considerable quantity of coagulated blood was effused

into the cellular tissue in the neighbourhood of the fractured portion of the tibia, which was still of a black colour, and presented all the appearances of a recent effusion. A small quantity of black blood was also found in the anterior mediastinum, around the part where the sternum had been fractured. The fractured ends of the sternum and tibia were firmly united. Circumstances might occur where the question may be raised, whether or not it is possible that different coagula of extravasated blood, varying so much in colour as in the case we have given above, could be the result of injuries inflicted at the same time. Though there could be no doubt that this patient received, at the same time, the different injuries we have mentioned, yet the blood which had been effused in consequence of these, had undergone very different degrees of absorption in different parts, as indicated by its appearance. The process of absorption had been most active from the inner surface of the serous membrance (arachnoid,) and least active in the cellular tissue.

In a boy, aged eighteen, who received a severe injury from a railway waggon passing over the pelvis and lower part of the abdomen, and who died twenty-three days after the occurrence of the accident, from the sloughing of the integuments and subjacent parts, the blood effused into the cellular tissue presented the following appearance :—On opening the abdomen the subserous cellular tissue over the bladder, the inner surface of the pelvis, and the lower part of the abdomen, was infiltrated with a dark-blue matter which appeared to be intimately incorporated with it, and was nowhere collected into separate masses.

Cases of Cerebral Disease.—The following case is a very important one, for if the patient had received a blow during a scuffle, and not from an accidental cause, it might have led to most important consequences, if those who were

employed to examine the body had failed to interpret aright the morbid appearances, and to discern their true character :—

Case VII.—W. B. aged twenty-two, admitted 15th June, under the charge of Professor Syme. He was of somewhat dissipated habits, was muscular, and as far as could be judged from external appearances, had enjoyed good health. About an hour and a half before his admission, he received a blow on the right side of the face, from the fall of a small bale of cotton. The blow was described to have been slight, and there were no signs of external injury. He was stunned by the blow, and fell down, but he immediately recovered, and remained sensible for a quarter of an hour, during which he walked about the distance of a gunshot to his own house. After this he again became insensible, and continued in that state up to the time of his admission. When admitted, his face was flushed; the pulse was ninety, and full; the respiration heaving; and the pupils dilated, and insensible to light; and he could not answer questions. A few minutes after admission, he was seized with a convulsive fit, which lasted four or five minutes. After recovering from this, he became exceedingly restless, remaining scarcely a moment in one position, and exhibiting a constant tendency to turn round and round on all fours. In general the attempt was a rotatory one, and was sometimes from right to left, at other times from left to right. This continued from eight o'clock in the evening till five o'clock next morning, and was followed by delirium. Next day the head was leeched, and he was bled from the arm. On the 17th the delirium greatly subsided, and he answered questions rationally. On the 18th the delirium returned, and it was necessary to put him under restraint. The delirium continued, and his strength began to fail, and he died on the morning of the 22d.

Sectio Cadaveris, 23d.—No effusion of blood into any of the tissues of the scalp, and no fracture of skull. Some serous effusion under the arachnoid, and in the sulci between the convolutions, but not sufficient to elevate that membrane above the surface of the convolutions, except at the most depending part. No flattening of the convolutions, or pressing of the sulci together. On taking a slice from the upper part of the left hemisphere of the brain, though it did not extend to half an inch in depth, the lateral ventricle of that side was opened into, and a quantity of pure limpid fluid flowed out with a sudden gush. The lateral ventricle of the opposite side was equally distended; the septum lucidum was much stretched and reticulated at the posterior part. There was no softening of the inner surface of the ventricles, nor of any other part of the brain; and there were no appearances of inflammation to be observed on the inner surface of the ventricles. The substance of the brain was of its usual consistence, and the only thing which appeared anormal when slicing the brain, was a greater quantity of blood in the vessels of some part of the brain, than in others. The quantity of fluid in the lateral ventricles might be between three and four ounces.

It would appear that this man received a severe injury on the head eighteen months before his death, and his wife has stated, that for some time before he received his last injury, he was restless at night, and manifested this tendency to turn round.

We were fully convinced that the morbid appearances observed within the cranium, in this case, were not produced by the blow which the patient received shortly before his admission into the Infirmary, but had been going on slowly and gradually for a considerable time, probably commencing eighteen months before, when he received the severe injury. We arrived at this conclusion on the following grounds:—We are satisfied that, if an effusion of

serum to the amount of between three and four ounces had taken place into the lateral ventricles in a young athletic person, in the short space of seven days—the length of time which elapsed between the receipt of the last blow and his death—the appearances observed would have been very different from those we have described. We believe from what we have observed, that it is impossible for so large a quantity of fluid to be effused in so short a time into the lateral ventricles, without producing that smoothness of the external surface of the brain, that flattening of the surface of the convolutions of the brain, and the pressing of these together by which the sulci between them become indistinct or almost obliterated, so characteristic of rapid effusions into the lateral ventricles. In such cases we have conjoined with the morbid changes just mentioned, dryness of the surface of the brain, and a glistening appearance of the arachnoid membrane covering it; and also generally some softening of a greater or less portion of the substance of the brain, or some other signs of recent inflammation, either in the interior of the brain, or in its membranes. Here, however, there was some serous effusion under the arachnoid, and in the sulci between the convolutions—there was no flattening of the convolutions, nor pressing of them together, the serum in the lateral ventricles was quite limpid, and there were no traces of recent inflammation. The opinion that the anormal appearances observed in the brain were anterior to the last blow he received on the head, is further strengthened by the statement of his wife, that for some time before this he was restless during the night, and manifested a tendency to turn round. The same, or symptoms very similar to those observed in this patient, to succeed the blow, would in all probability have subsequently manifested themselves without any injury of the head, but their occurrence was in all likelihood hastened by the effects of the blow. If this large quantity of fluid

in the lateral ventricle, and the other appearances described, had been found in the brain of an aged and infirm person, or in an individual emaciated and debilitated by long continued disease, we would feel considerable hesitation in attributing these to the results of a morbid action, for it is not unusual, under the circumstances mentioned, to find considerable quantities of fluid both in the lateral ventricles and in the sub-arachnoidian cellular tissue, without being attended by any cerebral symptoms during life. We believe that the explanation of these appearances in such cases is, that the absorption of the cerebral substance proceeds more rapidly than these changes in the cranium, by which it accommodates itself to the size of the brain, and a quantity of serum escapes from the blood-vessels to occupy the space left by the removal of part of its solid contents. We do not, however, find this increased quantity of serum within the cranium in all aged and infirm individuals, nor in all those much emaciated by long continued disease. We have collected, and are still collecting, a great number of facts upon this question, which will probably appear in some future Number of this Journal.[1]

This case is also interesting in a physiological point of view, as the patient manifested a tendency to perform the rotatory motions observed by Magendie after direct injury of one of the crura cerebelli, and also from vertical sections of the cerebellum, including one of its crura. This case, however, like others we could mention, afforded no evidence in favour of Magendie's views on this subject, for the crura cerebelli and cerebellum were very carefully examined, with a reference to this question, and no anormal appearances could be there detected.

Case VIII.—A young woman, aged nineteen, who acted as a nursery-maid in the family of a gentleman, began to

[1] Published in April Number, 1843.

complain of slight dyspeptic symptoms, and her abdomen had increased in size. At the time of her death the menstrual discharge had not appeared for seven months, and the abdomen had acquired a considerable size, so that it was generally believed by her neighbours that she was considerably gone with child. She occasionally complained of slight pains in the abdomen, and of transient headachs, but they were never so severe as to attract much attention, nor prevent her from going about her usual duties. On the morning of her death she complained that she felt sick, and was advised to go to bed. She went to her room apparently for that purpose, but returned in a short time and stated that she again felt quite well. She then went out to walk with the child under her care, and shortly after returning, she was suddenly seized with convulsions and insensibility, and was dead in a quarter of an hour. *Inspection twenty hours after death.*—On removing the skull-cap, the dura mater was found tensely stretched over the brain. The surface of the brain was smooth, the convolutions were pressed together so as nearly to obliterate the sulci, and the surface of the brain was dry and glistening. On removing a thin slice from the upper part of each hemisphere, the posterior part of the left was at least three quarters of an inch broader than the corresponding part of the right hemisphere, and on touching it with the finger distinct fluctuation was felt. On making a deeper section, a quantity of bloody serum flowed out from the posterior part of the left hemisphere, and a large clot of dark-coloured coagulated blood came into view, mixed up with some broken-down pieces of brain. This clot would have nearly filled an ounce measure, and extended from near the posterior surface of the left hemisphere to the outer edge of the corpus striatum of the same side, and approached closely to the lateral ventricle, but had not opened into it. The substance of the brain in the immediate neighbourhood of the

upper and outer surfaces of this clot was of a pale cream-yellow colour, and almost defluent (yellow inflammatory softening of the brain). The rest of the brain was of a pure white colour, but of normal consistence. The arteries of the brain were not diseased. The uterus was empty, but its outer surface, and that of all the other abdominal viscera, were covered with nodules of firm lymph, and several pounds of serum were contained in the cavity of the peritoneum.

I have inspected the bodies of two other females who died from apoplectic effusion of blood, consequent upon inflammatory softening of the brain. Both of them were seized with insensibility and convulsions, without any premonitory symptoms, except that one of them had complained of a slight headach a very few hours before the commencement of the attack. One of these was thirteen and a half years of age, and died three hours after the commencement of the attack. The other was thirty-three years of age, unmarried, and was nearly seven months gone with child, and died nine hours after the commencement of the attack. In another case the softening of the brain proved fatal, without any effusion of blood. This woman was about thirty years of age, and presented herself in the waiting-room of the Royal Infirmary about eleven o'clock, A.M., and stated that she had suffered from headach for about a fortnight or three weeks. About an hour after this she was found in a state of insensibility, and in a few minutes all signs of life had disappeared. A considerable portion of the left hemisphere was softened, was of a light yellow or cream-colour, without any surrounding redness, or any other change. These patients must have been going about with a latent disease of the brain, which in three of them was not indicated by any symptoms, and were liable each moment to the occurrence of the most alarming and dangerous attacks on exposure to any cause of excitement.

CASE IX.—*Case of Diaphragmatic Hernia produced by a penetrating wound.*—W. R., aged forty-five, a shoemaker, was admitted, on the 13th September 1838, into the Infirmary, under the charge of Dr. Shortt, about noon. He stated that he had been seized about four o'clock, P.M., of the previous day with vomiting, succeeded by dyspnœa, and that the latter had continued to increase up to the time of his admission. He also stated, that for upwards of a year he had been subject to occasional severe pain in the left hypochondriacal region, and also to cough without any expectoration. On admission he complained of great pain in the left hypochondrium, severe dyspnœa, with a depressed anxious expression of countenance; the pulse was intermittent, and so weak that it could not be reckoned. Extremities cold, and lips livid; percussion dull over the whole of the left side of the chest, and the respiratory murmur was there inaudible, while it was puerile on the right side. The sounds of the heart were only audible under the cartilages of the fourth, fifth, and sixth ribs on the right side. Had wine ordered, and sinapisms were applied to the chest and feet. Died in four hours after admission.[1]

Sectio Cadaveris, 16th September.—Heart healthy, but somewhat displaced towards the right side. Right lung sound. The left side of the chest contained more than six pounds of a reddish fluid, but there was no recent effused lymph on the pleura. The left lung was compressed towards the spine, and also towards the edge of the cordiform tendon of the diaphragm to which it adhered; and the left side of the diaphragm was pushed downwards into the abdomen. The lung itself was devoid of air. A dark soft mass was seen lying in the lower part of this side of chest, and was connected to the upper surface of the diaphragm.

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[1] The above account of his symptoms on admission was furnished by Dr. Alexander Wood.

On examining the upper part of the abdomen, and the lower surface of the diaphragm, it was obvious that the dark mass observed in the chest was composed of a part of the transverse arch of the colon, and a considerable mass of the large omentum, which had passed through an opening with callous edges, in the diaphragm, and become strangulated. The parts were now carefully examined *in situ*. A cicatrix nearly half an inch in length was observed in the skin at the lower part of left side of chest, midway between the anterior and posterior extremities of the ribs; and, on dissecting off the skin, a similar cicatrix was found in the ninth and tenth intercostal space, exactly opposite to that in the skin. The same cicatrix was distinctly seen on looking at the inner surface of the chest, and a process of that part of the large omentum which had accompanied the transverse arch of the colon through the diaphragm, not only adhered to the edge of the cicatrix, but was incorporated with it, and projected into the intercostal space. The aperture in the diaphragm would have admitted the passage of the points of three fingers with difficulty, and was filled up by the protruding and returning portions of the transverse arch of the colon, and was situated between the last left rib and cordiform tendon of the diaphragm, and between two and three inches from the origin of the muscular fibres of the diaphragm from the last rib, and was on a line with the cicatrix in the intercostal space, but placed on a lower level when the diaphragm was depressed. On the other hand, when this muscle was pushed up, in a manner similar to what must occur, when it is in a relaxed state or in the act of expiration, the opening in the diaphragm was then brought on a level with the cicatrix in the intercostal space, and considerably approximated to it. That part of the transverse arch of the colon which lay within the chest was at least a foot in length, was considerably dilated, and much thickened in its coats, was of a deep dark colour, was

soft in several places, and at one point had given way. The two portions of the transverse arch of the colon which lay in the opening of the diaphragm were considerably constricted. The entering portion (or the portion next the *caput cæcum*) could still be drawn upwards and downwards, and the little finger introduced into the interior of the tube could be pressed upwards, though with some little difficulty, through the opening in the diaphragm. The returning portion of the transverse arch of the colon was connected to the margin of the opening in the diaphragm through old adhesions of the omentum.[1]

It was stated by a friend of this man, who was present at the inspection, that he had received a wound about fifteen months ago, in the lower part of the left side of the chest with a shoemaker's knife, in a quarrel with a woman with whom he cohabited, and that she was liberated from prison after his apparent recovery. Between the time of his receiving the wound and his death, it appears that he was at two different times a patient in the Infirmary with symptoms of severe ileus, and was each time dismissed apparently cured.

Though an examination of all the circumstances connected with this case leads us to the conclusion, that the protrusion of the transverse arch of the colon into the cavity of the left pleura took place in consequence of a wound made in the diaphragm, and that this was the cause of death, yet there are various considerations, arising from the accounts given by authors of cases where a greater or less number of the abdominal viscera had passed into the thorax from a congenital deficiency in the diaphragm, which ought to be carefully weighed before we can venture to give a definite opinion in similar occurrences, as this may involve most important consequences. Doubts will,

[1] The preparation is preserved in the University Museum.

and ought, under such circumstances, to exercise an amount of influence on our decisions, which we might not be disposed to allow them in ordinary cases. The fact that the omentum not only adhered to the edges of the cicatrix of the wound observed on the inner surface of the thorax, but was also incorporated with it, and projected into the intercostal space, is sufficient to prove, that the omentum was present in the left side of the chest during the time that the wound was healing; but it is not of itself sufficient to decide the question which may be raised, as to the possibility of the passage of the intestine into the chest at some period previous to the infliction of the wound.

1. The first consideration which naturally presents itself on examining the recorded cases of the passage of some of the abdominal viscera into the thorax from congenital deficiency of part of the diaphragm, is the circumstance, that, though a considerable number of these have been observed in infants who were either still-born, or who died within a few weeks after birth, and had oppressed breathing from the moment of birth up to their death; yet there are others where the individual lived several years, and in some cases even to an advanced age, without any suspicion having been entertained that such a displacement of the abdominal viscera existed. In some of these, however, as we shall immediately see, the respiratory function was more or less embarrassed.

Riverius[1] relates the case of a young man, who, when slowly convalescing from intermittent fever, took an antimonial emetic from an empiric, and after ineffectual efforts to vomit, died some hours after. On dissection, the stomach was found in the right side of the chest, and the lung of that side was defective. Up to the time of his death this

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[1] Opera Omnia Medica. Observ. Centuria Quarta, Obser. 67, p. 549. Lugd. 1690.

young man had experienced no dyspnœa, had enjoyed good health up to the period of his last illness, and had served as a soldier. G. T. Weyland[1] has minutely detailed the case of a boy seven years of age, who had been affected with frequent vomiting from his infancy. He appeared to have no other complaint, but this from its frequency rendered his body thin and imperfectly nourished. He was not observed to have any difficulty of breathing, even when playing with his companions. When he was seven years of age the vomiting became so frequent, accompanied by pain of the head and abdomen, and his health was suffering so much that a physician was sent for. After being subjected to treatment for about twelve days, he began slowly to recover his strength. Fourteen days after this he had a relapse after exposure to cold, and he died. On removing the sternum, the left side of the chest was seen to be filled with the folds of the intestines as high as the second rib, and the left lung was consequently much diminished in size, and devoid of air. The stomach was placed in the abdomen, was of great size, and lay in a vertical position, its pyloric extremity extending downwards to the pelvis. Dr. Monro *Tertius*, has detailed two very interesting cases of this kind. One was a female twenty-two years of age, who died with symptoms of internal strangulation, and on inspection of the body a large portion of the arch of the colon was found to have passed through a small opening in the left side of the diaphragm. She never had any difficult breathing, but was subject to pain in the lower part of the left side. The other was a male of middle age, who died with cerebral symptoms, accompanied with difficult breathing and expectoration, and the arch of the colon and the omentum were found in the left side of the chest. He had an attack of

[1] Dissertatio Inauguralis Medica Duos Exhibens Casus Dislocationis Viscerum nonnullarum Abdominis. Jenae, 1831.

ileus four months previous to his death.[1] Bartholin[2] and Clauder[3] relate the case of a man who had always enjoyed a free and easy respiration, except a temporary fit of asthma, but who had long laboured under vomiting and constipation, in whom the stomach, the duodenum, part of the colon and omentum, had passed through a large opening with callous edges into the left side of the thorax. Petit[4] details the case of a man who had colic pains and difficult breathing for forty years, and on dissection a great portion of the colon, of the omentum, and of the splenic extremity of the stomach, had passed into the left side of chest. The protruded parts had contracted no adhesions, and were not covered by a hernial sac. Sir Astley Cooper[5] relates the case of a woman, who died, when twenty-eight years of age, with symptoms of inflammation of the abdomen of a few days' standing. On dissection, about eleven inches of the transverse arch of the colon, and a great part of the omentum, had passed through an opening in the diaphragm, into the left side of the chest, and the omentum adhered to the aperture in the diaphragm. Dr. John Clark[6] details the case of a man who died, when forty years of age, with all the symptoms of peritoneal inflammation, and on dissection, a part of the transverse arch of the colon, and part of the left lobe of the liver, passed through an opening in the diaphragm, which he supposed to arise from congenital malformation of that muscle. This man had enjoyed good health up to the two last years of his life. As he had received a fracture of two of his ribs a year before his death,

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[1] Morbid Anatomy of the Gullet, Stomach, and Intestines, p. 180. 2d edit.

[2] Boneti Sepulch. Anat., tom. ii., p. 803.

[3] *Vide* Sepulchretum, et Morgagni de Sedibus et Causis Morborum, Lib. iv., Epist. 54, Sec. 11.

[4] Maladies Chirurgicales, tom. ii., p. 161. 1783.

[5] Medical Records and Researches. London, 1798.

[6] Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. ii., p. 118.

it may be argued that a rupture of the diaphragm may have occurred at that time; yet, as it is stated that he recovered from the injury without any unusual symptoms, the case may at least be adduced as additional evidence of the fact, that part of the abdominal viscera may be present in the chest without any symptom to indicate their presence. Chauvet[1] states, that on examining the body of a lieutenant-colonel, he found the stomach and colon in the left side of the chest. And Vetter[2] observed in a very old person the whole tract of the small intestines lying in the left side of the chest.[3] Along with the above cases we may also include other three, in which part of the abdominal viscera appear to have protruded upwards into the thorax through the interval which naturally exists between the outer edges of the muscular fibres of the diaphragm which arise from the enciform cartilage of the sternum, and those which arise from the cartilages of the ribs. One of these is related by Morgagni.[4] It occurred in a man about fifty years of age, who died from a fracture of the skull, and the protruding part was the colon. A second case is related by Sir Astley Cooper,[5] on the authority of Mr. Bowles, and in this the right extremity of the stomach, the beginning of the duodenum, and part of the omentum, protruded into the right side of the chest, and were covered by a hernial sac formed by the peritoneum and the pleura. This man was also fifty years of age, and subject to asthmatic attacks, and died from excessive vomiting, after the exhibition of an eme-

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[1] Histoire de l'Académie Royale des Sciences de Paris pour 1729, p. 14.

[2] Aphorismen aus der Pathologischen Anatomie, S. 144, as quoted by Weyland.

[3] Chauvet says nothing about the state of the respiratory and digestive organs during life in the case which he relates; and Weyland does not inform us whether the previous history of the case by Vetter was ascertained.

[4] De Sedibus et Causis Morborum, Lib. iv., Epist. 54, Sec. 11.

[5] Opus. cit., p. 14.

tic, as occurred in the case related by Riverius.[1] Another case, where the diaphragmatic hernia protruded through the interval on the right margin of the fibres arising from the enciform cartilage, is related by Professor Bignardi.[2]

After a knowledge of the above cases, it could not be argued that, if the protrusion of the transverse arch of the colon, in the case we have detailed, had arisen from congenital deficiency in the diaphragm, it must have manifested itself before the reception of the wound, by some embarrassment of the respiratory function. The fact, moreover, that this protrusion must have existed from the time of the infliction of the wound up to his death, without any marked impediment to the respiration, is obviously quite sufficient to invalidate any similar argument.

2. In the case we have related the protrusion of the colon through the diaphragm had occurred on the side of the chest in which it is generally found in cases of deficient formation of the diaphragm. Of twenty-four recorded cases (not including the last three referred to) which I have examined for this purpose, I find that in three only had the protrusion happened on the right side. One of these is the case of Riverius, already referred to; another is one of the two cases mentioned by Dr. Macaulay;[3] and one by Bonn.[4]

3. The congenital deficiencies in the left side of the diaphragm, not only vary in extent, but also in position. In several of the recorded cases, the deficiency appears to

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[1] Petit (opus cit.) relates the case of a man long subject to dyspnœa, and who had been treated for asthma, in whom the stomach, the colon, and the omentum, were pushed up into the left side of the chest, and were enclosed in a sac formed by the diaphragm, peritoneum, and the pleura. See also Beclard's Supplement au Traité de Scarpa, p. 132, for two cases where the sac was formed by the pleura and peritoneum.

[2] Sull' ernia diaphragmatica, 1827, as quoted by Laurence.

[3] Medical Observations and Inquiries, vol. i., p. 26. London, 1763.

[4] Descriptio Thesauri Ossium Morbosorum Hoviani, No. 204, p. 69, as quoted by Laurence.

have included the whole of the left side of the diaphragm, and in others it is merely stated that the opening was on the left side. In two cases the opening was placed in the cordiform tendon, (case of Petit, and one of the cases of Weyland;) in one it was stated to have been near the opening for the *cava ascendens*, (one of Vetter's cases); in one the protrusion is said to have occurred through the opening for the sympathetic nerve, (Platner's Disput. De Hydrocel, as quoted by Morgagni;) in three the protrusion occurred through the œsophageal opening (Resigius de Ventriculi in Cavo Thoracis Situ Congenito, Berlin, 1823;) a case by Fantoni, as quoted by Monro, and a case by Clauder; in one it was placed an inch to the left of the œsophageal opening, (one of Dr. Macaulay's cases;) in one, it was placed three inches to the left of the œsophagus, (case of Sir A. Cooper;) in one, in the middle of the left portion of the diaphragm, (one of Monro's cases;) and in one (supposing it to be a case of congenital hernia) it must have occupied nearly the same situation as in the case we have described, since it is stated to have been situated "about three inches from the ribs, and placed more anteriorly than the œsophagus."

4. In at least two of the recorded cases the protruding parts were firmly adherent to the edges of the opening, (cases of Sir A. Cooper and Chauvet.)[1] We would certainly expect that in cases of wound of the diaphragm, that if the person survived, inflammation of the edges of the divided muscle would occur, and probably cause adhesions between the edges of the wound and the parts which may have protruded into it, as in the case we have described. That lymph may be effused and unite the protruded parts to the edges of the opening in some cases of congenital

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[1] In Sir A. Cooper's case, the omentum was the adhering part.

deficiency of a part of the diaphragm, is *a priori* certainly quite possible, and in two cases which we have cited above it had actually occurred.

5. Some of the cases of congenital deficiency of the diaphragm terminated fatally, as in ours, by strangulation of the protruding portion of the intestines, (cases of Dr. Clark, Sir A. Cooper, and Monro.)

Approaching the consideration of this case with all the caution which an examination of the recorded cases of congenital malformation of the diaphragm must naturally induce, we still feel strongly convinced that the protrusion of the colon was the consequence of a wound of the diaphragm. If we reflect upon the circumstance, that a sharp instrument could scarcely be thrust into the chest through the ninth and tenth intercostal space, where the cicatrix was found, without wounding the diaphragm, and thus necessarily at the part where the opening in it was found, if the muscle be in a relaxed state, or during expiration, as we have frequently satisfied ourselves upon the dead body; upon the great rarity of such small congenital deficiencies in the diaphragm; upon the uneasiness felt by the patient in the left hypochondrium, and the severe attacks of ileus after the infliction of the wound; conjoined with the improbability that a person could have arrived at the age of forty-five without fatal strangulation of the intestine, and have acquired an athletic form of body, when that part of the intestine passing through the opening would with difficulty admit the passage of the little finger, even when the returning portion was empty—we can scarcely suppose that the conjunction of the opening in the diaphgram and the infliction of the wound in the parietes of the chest, were in this individual a mere coincidence. It ought not, however, to be concealed, that a more accurate account of the previous history of the patient than we are able to give would have been very desirable.

Believing that the protrusion of the intestine into the chest was in this case the consequence of a wound in the diaphragm, there are some cases on record which bear to it a greater or less resemblance; and it is of some importance to know that the accounts given of them do not discountenance the idea we have formed of the nature of this case. Sennertus relates, in a letter to Hildanus,[1] the case of a man who stabbed himself beneath the ninth left rib, and died about seven months afterwards. On dissection the stomach was found to have protruded through the cordiform tendon into the left side of the chest.[2] In this case the seat of the wound in the parietes of the chest must have been placed nearer the sternum than in the one we have detailed. Ambrose Paré[3] relates the case of a person who died eight months after receiving a penetrating wound in the chest, and in whom a large portion of the colon was found in the cavity of the left pleura. This person had suffered from colic pains after the reception of the wound. Mr. Boyle[4] gives the case of a soldier who received a wound in the chest eleven months before his death. He died with symptoms of peritoneal inflammation, and a great part of the ileum and transverse arch of the colon had passed through an opening in the diaphragm, and become strangulated. From the time he received the wound "the respiration was affected, and even moderate exercise was supported with difficulty." Mr. Greetham[5] has published a case of a muscular man, between thirty and forty years of age, who had been wounded by a knife some years before in the left side of the chest, and who died with all the symptoms of strangulated intestine. On dissection, the

[1] G. F. Hildani, Medico-Chirurgi, Obser. et curat. Chirurg. Centuriæ, Cent. ii., Observ. xxxiii. 1541.

[2] *Vide* Morgagni de Sedibus et Causis Morborum, Lib. iv., Epistol. 54. Sec. 12, for other cases.

[3] Lib. 9, Cap. 30. [4] Vol. viii. of this Journal. [5] Medical Gazette, vol. x., p. 43.

omentum and a part of the colon had protruded through an opening in the cordiform tendon of the diaphragm. He had at times severe pains in the stomach after the infliction of the wound; but he had been able to act as a steward in a vessel during several distant voyages. The larger curvature of the stomach was drawn upwards towards the opening in the diaphragm by the omentum in the chest, so that it was nearly reversed.

Dr. M'Crie[1] has given an account of a preparation preserved in the Chatham Museum, obtained from a soldier who had been wounded twenty-two years before, and who died of gangrene of the lower extremities. In this case the stomach and a great part of the transverse arch of the colon had passed into the chest. After the reception of the wound up to the time of his death, he laboured under dyspeptic symptoms, and his breathing was affected on walking fast, or on ascending a hill, but these did not prevent him from acting as a sergeant in his regiment.

It would also appear that similar effects may result from laceration of the diaphragm from a fall. Dessault[2] relates the case of a man who received a fall when thirty-nine years of age, and who lived four years after this, in whom the stomach and arch of the colon had passed through an opening with callous edges into the left side of the chest. Though he was able to return to his usual employment after his recovery, he continued to complain of pain of chest and oppression of the breathing. Mr. W. D. Morgan has described the history of a patient who died in the Bristol Hospital, in whom several parts of the alimentary canal were found within the left side of the chest. The opening in the diaphragm was attributed to a fall upon his back received thirty-eight years before. From the time of the

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[1] Medical Gazette, vol. xv., p. 872.

[2] Journal de Chirurgie par Dessault, tom. iii.

injury to his death he was able to follow his usual occupation as a mason, but had often suffered from asthmatic dyspnœa, dyspepsia and constipation.[1] A very remarkable case is related by Mr. Taylor,[2] of a man who had received many years before his death, a fall in which some of the left ribs were fractured. On dissection, a great part of the left side of the chest was filled by the stomach, and a large part of the transverse arch of the colon, yet he was able to perform the usual duties of a sailor up to his last illness, and the respiration appears to have been little affected. This man died after amputation of a leg.

[In the Edinburgh Medical and Surgical Journal, No. 144, I have published two cases of diaphragmatic hernia resulting from external injuries.]

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[1] Medical Gazette, vol. xii., p. 673.
[2] Guy's Hospital Reports, vol. iii., p. 366.

No. XXIII.

ON THE VALUE TO BE ATTACHED TO EXPERIMENTS ON THE NERVOUS SYSTEM.

In the preceding paper I had occasion to express my distrust of the accuracy of some inferences drawn by Magendie from the results of his experiments on the effects of lesion of certain parts of the encephalon on the movements of the muscles of voluntary motion. Some writers have taken advantage of the unsatisfactory nature of the inferences which have been drawn from these and similar experiments, to throw discredit upon *all* experiments upon the nervous system. While we readily admit that some physiologists have shown a want of proper caution in their conclusions regarding the functions of particular parts of the nervous system from the results that follow their lesion, it must be remembered that these errors have arisen from ignorance of the nature of the evidence which such experiments are capable of furnishing. Experiments on the nervous system are very likely to lead to erroneous conclusions, especially such as those upon the cerebellum and other parts of the encephalon, where a correct appreciation is wanting of the value of the evidence they yield. No doubt the amount of trustworthy evidence afforded by experiments on the central organs of the nervous system is very circumscribed, but that they are capable of furnishing unobjectionable proofs of the particular functions performed by individual parts of the central organs of the nervous system, can be

undeniably established. As a considerable part of the physiological inferences contained in this volume rests upon experimental evidence, I have here given the following remarks on this subject, forming part of a lecture on the nervous system, delivered several years ago.

"It appears from the experiment above mentioned that serious injury and removal of the cerebellum were generally attended by various derangement of the voluntary movements. That this should have been the case need not surprise us, when we remember that in experiments of this kind the walls of the cranium must be partly removed, and the interior of the cavity laid open, and that the incisions necessary for the removal of a portion of the cranium, and in slicing away the cerebellum, must be attended with effusion of blood, which, by its pressure, cannot fail to exercise very injurious effects upon the medulla oblongata, and other parts of the encephalon. And this is not all; for it is obvious that this cannot be done without inducing derangement of the circulation within the cranium, and various serious sympathetic disturbance of other parts of the encephalon, though placed far beyond the reach of the operator's scalpel or actual cautery. The effect of the combined agency of all these causes in deranging the functions of the encephalon can easily be imagined when we remember by what apparently trivial circumstances this is frequently induced. In judging of the value to be attached to such experiments, we must never forget that the physiologist is obliged to pursue his investigations into the functions of a complex organized body under very different circumstances from the mechanical philosopher and the chemist, who can generally isolate the substances upon which they operate, and remove all other influences which might affect the accuracy of the results. The physiologist, on the other hand, does not operate on inanimate materials which

he can separate and arrange at will, but upon a living and complicated mechanism, the movements of which are so interwoven, and have such reciprocal influence upon each other, that in examining the action of any particular agency upon any individual organ, it becomes exceedingly difficult to ascertain to what extent the phenomena which result are to be attributed to its effects upon that organ, and to what extent these are dependent upon changes which have been induced in the functions of some of the other organs of the body. And when we attempt to increase our acquaintance with the particular functions of the different portions of certain organs, or set of organs, and more especially with the brain, we find these mutual reactions of the different parts of the organ upon each other carried to a most perplexing extent. In experiments performed upon living animals, we find that the most discordant results have been obtained from injuries inflicted upon the same portion of the brain in different animals of the same species, and similar results from injuries inflicted upon very dissimilar parts of the same organ. We also find that the same circumstances have hitherto baffled all the laborious attempts of physicians to connect particular symptoms with organic derangement of particular parts of this organ.

"If we allow these considerations to have their due weight, we will necessarily be very guarded in drawing inferences from experiments performed with a view to ascertain the functions of any particular part of the encephalon by ablation, or cauterizing it. No doubt, if, in a great number of experiments upon any particular part of this organ, certain functions were invariably deranged, and this too in the direct ratio of the injury inflicted, we would be inclined to admit that there is a strict relation between the integrity of that part of the organ and the manifestation of the function thus influenced; but as we never can be certain to what extent the injury done in the performance of the

experiment may affect the functions of other parts of the organ not included within the parts injured, it is necessary to test these results in a way which we are about to mention before we can place implicit confidence in them. We have seen that the disappearance of a function after the removal or injury of a particular portion of the encephalon, is no certain proof that this function is necessarily connected with that organ; but, on the other hand, if we find a particular function, the performance of which is attributed to a certain organ, still remain perfect after the complete destruction of that organ, then we have the most conclusive evidence that the performance of the function is not necessarily connected with the existence of that organ, for a substance cannot act where it is not. If, for example, we find the voluntary movements either imperfectly performed or arrested after destruction or removal of the cerebellum, we are certainly not entitled to conclude that the function of volition is dependent upon the cerebellum, for the effects of the incisions and hemorrhage necessarily attendant upon all such experiments may affect the functions of other parts of the brain; but suppose, on the other hand, we find the voluntary movements continue perfect after removal of the cerebellum, then we have the most conclusive of all evidence for affirming that the function of volition is not necessarily dependent upon the cerebellum, for it would be absurd to suppose that a function can be manifested without an organ. It must also be sufficiently obvious that the deep incisions, hemorrhage, and infliction of pain, cannot be urged against the results of this last experiment; on the contrary, all these would operate in favour of its accuracy, for if the function continued, not only after the organ supposed to perform it had been removed, but also after various other injuries had been inflicted, it affords, if possible, stronger confirmation of the independence of that function upon the presence of that particular organ. The former experiment, which we have here supposed, is

a negative one, and must always be received with the utmost caution; the latter is a positive experiment, and is deserving of implicit confidence. One positive ought to outweigh a whole host of negative experiments. It is on these grounds that we believe we are fully justified in maintaining that the cerebral hemispheres and the cerebellum are not concerned in the production of these instinctive and sympathetic muscular movements termed the excito-motory, since we find them remaining perfect in the anencephalous fœtus, and after the removal of those parts in the lower animals.

"It is only when a negative experiment has been repeated several times under varied circumstances, and invariably with the same results, and when we are at the same time enabled by positive experiments to analyze the functions of other parts to a certain extent, that sufficiently accurate data can be obtained to permit us to draw our conclusions with perfect accuracy. For example, if we cut the portio dura nerve, (it being previously ascertained that the sensibility and the volitional movements of the face depend upon the nervous system,) and then ascertain that the sensibility remains perfect, though motion is lost, we are entitled to draw the conclusion that the sensibility of the face is not dependent upon the portio dura, but we cannot be at first positive that the loss of voluntary motion over the muscles of the face is strictly dependent upon the division of this nerve, since some other circumstances may have induced this. If, however, in frequent repetitions of the experiment we invariably produced the loss of voluntary power, we begin to feel pretty confident that the voluntary movements of the face are connected with the integrity of the portio dura; and this amounts to conviction when we find, after experiments on the fifth pair, the only other nerve distributed in these parts, that the very opposite results follow, viz., loss of common sensation, with persistence of the voluntary muscular movement."

No. XXIV.

REVIEW.[1]

Sicheres Heilverfahren bei dem schnell gefährlichen Lufteintritt in die Venen und dessen gerichtsärtztliche Wichtigkeit. Von Dr. Ch. Jos. Edl. v. Wattmann, *Professor der praktischen Chirurgie, und der erste Chirurgischen Klinik-Vorsteher des Operations-Institutes an der k. k. Universität in Wien u. s. w.* Wien : 1843.

(FROM LONDON AND EDINBURGH MONTHLY JOURNAL OF MEDICAL SCIENCE FOR AUGUST AND SEPTEMBER 1844.)

The rapidly fatal effects of throwing a quantity of air into the blood-vessels of an animal have been long known. The first experiment of this kind has been attributed to Wepfer, who is said to have laid prostrate and killed an ox of stupendous size, by inflating with his mouth the jugular vein.[2] Redi, in a letter to Steno, written about 177 years ago, mentions that he himself had instantly killed, by the same means, two dogs, a hare, a sheep, and two foxes. Similar experiments were repeated with the same results by Heyde, Brunner, &c. Additional interest and practical

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[1] I have been induced to reprint the above Review of Professor Wattmann's Treatise on the Entrance of Air into the Veins, as I believe that I have there given a fair and complete account of our knowledge on this subject up to the time it was written.

[2] *Vide* Morgagni de Sedibus et Causis Morborum, epist. v.

importance were given to these facts by Ruysch, Valsalva, Morgagni, and others, who observed in some post mortem examinations, considerable quantities of a gaseous fluid in the vascular system, which they believed to be air, and suggested the idea that quantities of gas may be evolved in the vascular system during life, and kill by inducing apoplexy, or by arresting the movements of the heart. This opinion seems, however, to have gradually lost ground, and the experiments and observations which had given rise to it, had been for many years seldom referred to, even by physiologists, when Bichat published his well known *Recherches Physiologiques sur la Vie et la Mort.* In this work he made some startling statements regarding the smallness of the quantity of air capable of causing death; and gave a new explanation of its *modus operandi.* The carefully conducted experiments of Nysten[1] shortly followed, and fully exposed some of the errors into which Bichat had fallen. In 1818, this subject assumed a novel and important feature to the practical surgeon. Until this time the only evils dreaded by the surgeon from the wound of a vein was hæmorrhage, and the subsequent effects of the application of a ligature to the wounded vessel, when this was necessary; but a new danger, most sudden in its occurrence, and most alarming in its consequences, was now made known. In that year, a patient expired at the Hôpital Saint Antoine, in the hands of Dr. Beauchêne, while extirpating a tumour from the right shoulder and lateral and lower part of the neck, under circumstances which led to the belief that this was occasioned by the entrance of air into the vascular system through an opening in a vein. Cases of sudden death during operations attributed to this cause were afterwards reported as having occurred in France, in Germany, in this country, and in America;

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[1] Recherches de Physiologie et de Chimie Pathologique, 1811.

and, as generally happens when public attention is roused by new and unexpected results, there was for some time a tendency to extend their application beyond legitimate bounds, and sudden deaths were referred to this cause, which, under the exercise of greater care and discrimination, would necessarily have been excluded. In 1837 a keen discussion upon this question, in the Académie de Médecine of Paris, followed the communication of a case by Amussat, where sudden and alarming symptoms of dissolution occurred during the extirpation of a diseased mamma, attributed by him to the entrance of air into a wounded vein. On this occasion Velpeau, Gerdy, Blandin, and Mallé, not only challenged the accuracy of this explanation given by Amussat, but by a searching analysis of the history of the other cases of this nature previously recorded, demonstrated that some of them, at least, were so imperfectly observed and described, or were attended by phenomena so dissimilar to those remarked in the experiments on the domesticated animals, that the cause of the sudden death of the patient must either be considered a matter of doubt, or attributed to some other agency. Velpeau afterwards published a connected account of his objections in the *Gazette Médicale de Paris*, 2d February 1838, and though he and his supporters have occasionally manifested a certain degree of hypercriticism, and have not allowed to some facts their due weight, yet there can be no doubt that this discussion was of much service in correcting the tendency to attribute death to the entrance of air into the veins on insufficient grounds, and in fully disclosing certain imperfections in our knowledge of this subject. In this discussion the objectors naturally attached considerable importance to the fact, that the phenomena observed in the experiments upon the domesticated animals on the effects of air in the vascular system, were in general different from

those which had presented themselves in the alleged cases in the human species, and this must have drawn attention to the circumstance, that hitherto the conditions under which the experiments were performed on animals, were not the same as those under which this occurrence took place in the human species. In all experiments and observations then made upon the lower animals, the air was forced into the vein, either by a syringe, or by blowing with the mouth, with the four following exceptions: one imperfect experiment by Mery upon a dog;[1] a case observed by Verrier in 1806, and a similar one by Bouley in 1821, of the entrance of air into the jugular vein in the horse, when opened in phlebotomy; and the experiment of Magendie, to prove that when a tube, whose walls are capable of bearing off the atmospheric pressure, is introduced into the jugular vein, and carried a certain distance towards the heart, atmospheric air readily enters the vascular system.[2] To remedy this defect Amussat undertook a series of experiments upon what has been termed the *spontaneous entrance* of air into the veins,—which means, that an opening was made into a vein, and the air allowed to enter by it, through the agency of certain physical causes influencing the circulation of the venous blood within and in the neighbourhood of the thorax, and was not mechanically forced in by a syringe, or by blowing through a tube,—and these were repeated in presence of a commission of the Academy, of which Bouillaud was reporter. These and additional experiments were published by Amussat in 1839.[3] The only authors in this country who have written on this subject since the time of Dr.

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[1] Académie des Sciences, 1707.

[2] Leçons sur les Phénomènes Physiques de la Vie, 1836.

[3] Recherches sur l'Introduction Accidentelle de l'Air dans les Veins, 1839.

Brown Langrish, who made only one imperfect experiment,[1] are Dr. Cormack,[2] Sir Charles Bell,[3] and Mr. Erichsen.[4]

The treatise of Dr. Wattmann contains no new experiments upon the lower animals, and is chiefly occupied, as its title would lead us to expect, in examining the causes that favour the accidental entrance of air into the venous system in the human species, the modes of preventing this, the remedies to be employed when it does occur, its other surgical relations, and also its bearings upon Forensic Medicine. He points out that the surgeon, by employing a certain procedure, can in most cases at least, prevent the entrance of a fatal quantity of air; and he describes a new method of securing the wounded vein when not more than half its circumference is cut through, by which the further entrance of air is prevented, and the hæmorrhage arrested from such large veins as the internal jugular and subclavian, without very much diminution of the calibre of the vessel. He also gives the details of four very interesting examples of the accidental entrance of air into the veins, which occurred in his own surgical practice, and though one of these was published as early as 1823, they have hitherto been entirely unknown to those who have collected and commented upon such cases, in France and in this country. In giving an account of the physiological part of his subject, he is necessarily led to examine the results obtained by the different experimenters upon the lower animals,—from which source almost all our knowledge of it is derived,—and in doing this he shows a perfect acquaintance with all that has been previously published on the question.

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[1] Physical Experiments on Brutes, 1764.

[2] Prize Thesis. Inaugural Dissertation on the Presence of Air in the Organs of Circulation, Edinburgh, August, 1837.

[3] Surgical Essays, 1840.

[4] Edinburgh Medical and Surgical Journal for January 1844.

We shall now proceed to give a connected, and as complete a view as our space will permit us, of the present amount of our knowledge upon the points discussed in the volume before us, selecting from each author those facts and opinions we deem most worthy of credit. We are the more induced to do this, as we are convinced that our knowledge of this subject is now sufficiently advanced to enable us to attempt it with considerable confidence and satisfaction.

1. *Quantity of Air introduced into the Venous System necessary to cause death.*—We have already referred to the exaggerated notions of Bichat on this point, who asserted that a few bubbles of air introduced into the vascular system were sufficient to cause immediate death. The experiments of Nysten proved that unless a considerable quantity of air enters the blood-vessels it does not prove fatal. He estimates that the injection of from forty to fifty centimetres cubes of air (between 2·44 and 3·05 cubic inches English) in a small dog, and from 100 to 120 centimetres cubes (120 centimetres being equal to 7·323, and 100 centimetres to 6·102 cubic inches English)[1] in a large dog are necessary to cause death, even when injected quickly into the veins, and that larger quantities are required to kill more voluminous animals, as the ox and horse. Magendie states that certain animals admit enormous quantities of air to be briskly introduced into their veins, without perishing, and adds, "I remember having thrown with all the force and celerity with which I was capable, forty to fifty pints of air into the veins of a very old horse without his dying immediately, though he sank at last." A considerably less quantity than this is in general sufficient to kill a horse. In the first experiment related by Dr. Cormack, and performed upon a horse, a tube a

[1] A cubic centimetre is equal to ·061025 of an English cubic inch.

quarter of an inch in diameter was introduced into the left jugular vein, and the person who blew, filled his chest twice, and continued to blow for a minute, before the animal exhibited those symptoms of uneasiness which are generally the forerunners of a rapid death. Though all the other experiments of Dr. Cormack and M. Amussat show that the introduction of a considerable quantity of air into the venous system is necessary to produce death in the domesticated animals, it is yet impossible to fix the exact quantity requisite, and it clearly varies not only in different species of animals, but even in different individuals of the same species. Judging from the recorded cases of the accidental entrance of air into the human species, the presence of a smaller quantity of air is capable of inducing death in them, than in the lower animals. Barthelemy, finding that six horses, weakened by previous loss of blood, died after the entrance into the veins of four litres of air, and a seventh after six litres, calculates that a man weighing 136 lbs. would be killed by the introduction of two-thirds of a litre of air into the venous system.[1] Such a calculation, as Wattmann correctly remarks, resting upon the supposition that the human species differs only from the horse in weight, is not entitled to much confidence. The quantity of air necessary to induce death is modified by the rapidity and volume with which it enters the opening in the vein. The same quantity of air which kills when introduced rapidly, will fail to do so if introduced more slowly.

2. *Period of Death.*—When the air is forced largely and rapidly into one of the great veins near the heart, death very soon follows. In one of Dr. Cormack's experiments upon a dog, the animal died in twelve seconds, and in another upon a horse, death occurred in three minutes

[1] One litre is equal to 61·027 English cubic inches.

after the commencement of the inflation of air into the jugular vein. In eighteen animals subjected by Amussat to the forced and rapid introduction of air into the jugular vein, death took place in fifteen of these at the following periods :—In rabbits from fifty seconds to one minute ; in dogs, after two minutes ; in sheep, from one to two minutes ; in horses, after three, five, six, six and a half, thirteen, sixteen, and twenty-three minutes.[1] The differences in the period of death in these experiments are partly due to original and unknown differences in the vital constitution of the animals operated on, and to a greater extent perhaps, as Dr. Cormack and M. Amussat remarked, to the manner in which the air is forced into the vein. When the air is thrown in forcibly through a large tube into a vein near the heart, and care is taken to prevent the escape of part of the air through the opening in the vein by the sides of the tube, the death is more rapid and certain than when the tube employed is small, or when part of the air again escapes from the vein. All these observations upon the period of death when air is *forced* into the veins do not, however, admit of any direct comparison with those made in the instances of accidental entrance of air in the human species, the conditions under which the air passes into the vein being so different in the two classes of cases. The experiments of Amussat on the *spontaneous entrance* of air into the veins, were, on the other hand, made under circumstances resembling as closely as it was possible to do, the conditions under which the air accidentally enters the veins in the human species. His first series of experi-

[1] As Amussat, however, dates the moment of death from the cessation of the reflex movements of the orbicularis palpebrarum muscle from irritation of the membrana conjunctiva of the eye-ball, and not, as many other experimenters more correctly do, from the cessation of the sensorial functions and the respiratory movements, the period of death will appear to be somewhat later, than if the cessation of the sensorial functions and the respiration had been taken as evidence of death. Op. cit. p. 33.

ments were performed upon twenty-six animals, and of these twenty-four died and two survived. Five rabbits died after one and a half, two, three, and five minutes; seven of nine dogs experimented on, died after one, three, six, ten, sixteen, twenty-four, and thirty minutes, while the remaining two survived; two sheep died after nineteen and fifty-six minutes; ten horses after fourteen, fifteen, seventeen, twenty-six, twenty-eight, thirty-five minutes, one hour forty-four minutes, one hour fifty minutes, and two hours thirty minutes. One only of the horses resisted the effects of the entrance of air, and this apparently in consequence of the neck resting against a tree, and a clot of blood forming and blocking up the orifice in the vein. When the clot was removed the animal died after twenty-six minutes. In a second series of experiments, he previously abstracted a certain quantity of blood, and subjected the animals to the usual incisions required in some surgical operations before he permitted the air to enter the vascular system, and thus placed the animal still more nearly in the circumstances of those individuals of the human species in whom the accidental entrance of air into the veins occurred during a protracted operation, and after the loss of more or less blood. This series of experiments was performed upon sixteen animals, and only one survived. The dogs died after one, four, six, and twenty-five minutes; the sheep after seven and sixteen minutes; and the horses after three and a half, nine, twelve, thirteen, sixteen, and twenty-three minutes. Thus it appears, that the period of death was hastened considerably in those animals whose blood-vessels had previously been depleted by the abstraction of part of their blood. Judging from the details of the cases of the accidental entrance of air into the veins during operations on the human species, the presence of a smaller quantity of air is not only capable of proving fatal, but death occurs more rapidly than in the domesticated animal, and this has

been urged among other objections we shall afterwards notice, against this explanation of the cause of the death of those individuals. We will merely remark at present, that though this objection is one which demands consideration, it must at the same time be remembered, that the loss of blood, with the pain and moral effects attending a protracted operation, must favour the injurious effects of the passage of air into the vascular system, and that the period of death varies considerably, not only in different genera of animals, but in different individuals of the same species. Wattmann (p. 66) endeavours to show, that the more rapid death in the human species may be also aided by the following circumstances. "Man," he says, "has a larger brain than the lower animals, and requires for its nutrition a greater supply of blood; besides, the human brain manifests more complex functions than that of beasts, and this is a second reason why it requires a greater supply of blood. To effect these two objects, man has relatively a shorter neck, and the cerebral arteries and veins are wider than those of beasts. Further, man has the pre-eminence of the erect position, and the vital processes are in an especial manner more linked and dependant upon each other than in the lower animals, so that they are more readily and speedily brought to a stand when the circulation of the blood is disturbed. From the relatively larger size of the veins in the neck, the air may enter more rapidly and in greater quantity when they are wounded, and readily spreading itself over the vascular system, destroys the conditions necessary for the maintenance of the vital processes, and proves rapidly fatal."[1] We cannot admit that this explanation of the more rapid death in the human species is satisfactory, and our reasons for this will be better understood when we come to examine the causes of death in

[1] Op. cit., p. 23.

such cases. In the meantime we merely observe, that we are not aware that any interruption of the circulation or respiratory functions, provided that this be to the same extent in both, proves more rapidly fatal in the human species than in the lower animals, or, that in asphyxia, for example, the period of death is earlier in the former than in the latter. As we shall afterwards point out, the erect position does facilitate the entrance of air into the veins; but during an operation the patient is as likely to be in a recumbent as in the erect position, and in a number of cases the air entered by the subclavian or axillary veins. The size of the vein is only one—though no doubt an important one—of the conditions which favour the rapid entrance of a large quantity of air. Air may pass into the vascular system, in quantities sufficient to induce alarming disturbance of the heart and respiratory organs, and yet the animal may not only survive, but rapidly recover from its immediate effects, as has been sufficiently proved by the experiments of Nysten, Cormack, Amussat, and others.[1] Nysten found, however, in his experiments, that they subsequently died in the course of a few days from inflammation of the lungs, and Amussat observed the same thing in one of his experiments. The experiments of Cormack and Amussat have shown that this is not so constant a result as Nysten supposed. When an animal recovers from the immediate effects of the experiment, the air disappears from the vascular system in the course of three or four days. In some of the recorded cases of the accidental entrance of air into the veins in the human species, the patient recovered from its immediate effects even after the vital processes were almost brought to a stand, and in Malgaigne's case the patient died on the fourth day of a pulmonary affection, probably bronchitis (par accumulation d'écume

[1] Journal Hebdomadaire, tom. ii., p. 165.

dans les bronches.) Mr. Erichsen[1] states, that one of Roux's patients died on the seventh day from pneumonia, but on reading over the autopsy,[2] we are satisfied that the appearances described, indicated nothing more than the congestion of the depending parts of the lungs, so very frequently found after death. There may have been some bronchitis in the right lung, but we have no evidence that there was pneumonia.[3]

3. *Morbid Appearances.*—An exact knowledge of the state of the organs after death, is absolutely necessary to enable us to ascertain the manner in which the vital functions are arrested by the entrance of air into the vascular system; and will enable us to set aside at once some of the explanations that have been given of the cause of death. All experimenters whose results seem worthy of confidence, agree that the right side of the heart is always much distended with air and blood, whether the air be forced, or allowed to enter spontaneously into the veins. In general the blood is intermixed with the air, forming a froth; more rarely, a great part of the air continues free or unmixed with blood, as observed in a few of the experiments of Dr. Cormack and M. Amussat. The pulmonary artery is also full of frothy blood, except in some cases where the air is forced rapidly and in large quantity into one of the veins in the neighbourhood of the heart, as occurred in one of Dr. Cormack's experiments. In some cases the air is confined to the right side of the heart and the venous system; in others, the pulmonary veins, the left side of the heart, and the arterial system, also contain it. As this last point is of considerable importance in ascertaining the cause of death, we shall examine it at greater length. Nysten states that he never found the air in the arterial system; and as

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[1] Op. cit., p. 23. [2] Journal Hebdomadaire, tom. ii., p. 163.

[3] One of Wattmann's cases died of inflammation of the lungs.

he operated on dogs alone, this will be accounted for by the following details:—In nine rabbits experimented on by M. Amussat and Dr. Cormack, air was found in small quantity in the left side of the heart in two; and in seven it was entirely confined to the right side of the heart and the venous system. In five of these experiments it was allowed to enter spontaneously, and in three it was forced into the vein; and the two cases in which it was also found in the arterial system, occurred among the latter. In six sheep, M. Amussat found a small quantity of air in the left side of the heart in four, and none in two. In twenty-one dogs experimented on by Nysten, Cormack, Magendie, and Amussat, air was found in the left side of the heart in one only, and in very small quantity. In twelve of these it had entered spontaneously, in nine it had been forced, and it was in one of the latter where the air was forced in slowly, that the small quantity was found in the arterial system. In twenty-six horses experimented on by M. Amussat and Dr. Cormack, the air was confined to the right side of the heart and venous system in seven; it was found in small quantity in six, and in large quantity in twelve, in the left side of the heart and arterial system; and in some of the latter, the arteries of the brain were not only filled with frothy blood, but a portion of the air had escaped into the sub-arachnoidean cellular tissue. In seventeen of these the air had entered spontaneously; in nine it had been forced. The presence of air in the arterial system was proportionally more frequent in those cases where it was forced, than in those where it had spontaneously entered the opening in the vein. In all these cases, the quantity of air was always much greater in the right than in the left side of the heart; and Amussat found that even in all the cases where the air had entered spontaneously, in which the chest was opened *immediately* after death, the right side of the heart was distended with air and frothy blood, and sometimes to the

extent of causing the heart to fill up completely the interior of the pericardium. With regard to the statement of Barthelemy, that he had observed the heart after death, from the spontaneous entrance of air into the veins, collapsed and flabby; and the experiment related by Mr. Erichsen, where he found the heart nine minutes after death contracting regularly and vigorously, and *its ventricles not fuller than usual*, we strongly suspect some source of fallacy, for they are not only completely at variance with the well-conducted and numerous experiments of Amussat, but are very different from what we would expect. One source of fallacy readily suggests itself,—one which we know has led to similar errors, and it is this:—In laying open the chest after death, if any of the large veins at the root of the neck be wounded, the right side of the heart may unload itself through the wound in the vein. In the cases of death in the human species, from the accidental entrance of air into the veins, the air has been sometimes confined to the right side of the heart and venous system; at other times it was also found in the left side of the heart and arterial system. In the seven cases placed by Amussat in the *first* of the five groups into which he has divided the examples of the accidental entrance of air into the veins in the human species, air was found both in the right and left side of the heart, and in the arteries, in two;[1] it was either not present or not observed in the left side, in five;[2] and only a few bubbles of air were found in the left side of the heart, but none in the aorta, in one case.[3] As in the remaining case the patient did not die until the seventh day, the air had probably in a great measure disappeared, though bubbles of air were still found in the aorta and its

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[1] Cases of Beauchêne and Dupuytren, 1822.

[2] Delpech 1830, Ulrick, Roux 1836, Clement, Putegnat.

[3] Castara.

branches, but none in the arteries of the brain.[1] From these observations, it appears that frothy blood passes more readily through the capillaries of the lungs in the horse and in man than in dogs. Nysten mentions in his experiments, that the lungs were healthy; and Dr. Cormack and M. Amussat, who attended particularly to this subject, state that the lungs were not emphysematous, as has been alleged.

4. *Symptoms occasioned by the presence of Air in the Venous System.*—In experiments upon animals, it has been observed that when the air first reaches the heart, its contractions are at first more rapid, but when it arrives in larger quantity, they become slower and feebler, the respirations become quicker and heaving, the animal gives indications of uneasiness, falls down, occasionally utters cries of distress, and dies in convulsions. The contents of the rectum and bladder are frequently expelled at the time of death. In a very few cases only is death from this cause not preceded by convulsions. Amussat mentions one example of this.[2] In some of the cases of death in the human species, attributed to the accidental entrance of air into the veins, the period of death was not only earlier, but some of the symptoms which uniformly precede death, in the lower animals, either did not occur, or were not observed. The constitutional symptoms which, according to Wattmann, (p. 55,) rapidly follow the entrance of air into the veins in the human species, are the following:—" Speedy occurrence of syncope, which is either preceded by a cry, with the expression, 'I die,' 'I am dead,' 'I suffocate,' or by anxiety and tremblings, or without any such precursors. The syncope rapidly reaches such a degree, that all consciousness is lost, and the patient falls down, cold sweat breaks out on the forehead, and in a quarter of an hour,

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[1] Roux, 1832.

[2] Experiment 3, p. 11. Op. cit.

sometimes sooner, sometimes later, he is dead." The increased respiratory muscular movements that precede the loss of consciousness, and the convulsive movements that usher in death, so generally observed in experiments upon the lower animals, have been very seldom remarked in cases of accidental entrance of air in the human species. In the fifteen cases forming Amussat's first and second series of facts observed in the human species, convulsive movements were only remarked in five of these. In one of the five (that of Beauchêne) the patient had slight convulsions; in Ulrich's case, slight convulsions of the face, and then episthotonos followed the sudden attack of syncope; in Roux's case in 1836, the patient became pale, then syncope occurred, followed by convulsions; in Mirault's case, the patient became pale, and had tremblings, with tetanic succussions; in Goulard's case, there was paleness, with convulsive movements of face, hiccough, and after some minutes death. In these fifteen cases, the one which approaches nearest in the character of some of its constitutional symptoms to the experiments on the lower animals, was one of the two that occurred to Roux. At the time the sound indicating the entrance of air into a vein was heard, the patient uttered a plaintive cry, turned herself in an agitated manner in all directions in her bed, the inspirations became long and difficult, and the patient was apparently fast dying. She, however, revived, and died seven days afterwards. In some of the other cases, as in those of Castara, Delpech, and Barlow, the death is said to have taken place suddenly, without complaint, and without convulsive movement. In one of Dupuytren's cases (that of Nov. 1822,) the patient cried out, "I am dead," was soon seized with sudden tremblings, then sank down upon her chair, and fell without movement and without life. Several of the cases, as we might have expected, are very imperfectly reported; for it is not to be supposed that the surgeon or

his assistants should possess the coolness and time to watch narrowly the phenomena, when their minds were agitated by the threatened sudden dissolution of their patient, and their attention distracted by anxious attempts to save him.

5. *Cause of death.*—The older experimenters attributed death from the inflation of air into the veins, to distention of the right side of the heart, arresting its contractions, and some of them compared its condition to that of the bladder when over-distended with urine. Bichat maintained that death begins at the brain, and depends upon the circulation of frothy blood in the vessels of that organ. Nysten, Cormack, and Amussat have referred the death to the mechanical distention of the right side of the heart. Leroy and Piédagnel attributed it chiefly to emphysema of the lungs. Sir Charles Bell believed that the air, by circulating in the vessels of the medulla oblongata, annihilated the functions of that important portion of the central organs of the nervous system, and thus killed by arresting the respiratory muscular movements. Marchel de Calvi supposes that death is due to the action of the carbonic acid contained in the air,—a supposition which could be readily disproved by a reference to some of Nysten's experiments.[1] Bouillaud, in his report to the Academy, attributes the death partly to the distention of the right side of the heart, and partly to the difficulty of transmitting the frothy blood through the lungs; and Mr. Erichsen refers it entirely to the latter of these two causes. Wattmann (p. 70) gives at considerable length his explanation of the cause of death, which is much too long to transcribe. He attributes it partly to the disturbance and enfeeblement of the contractibility of the heart occasioned by the mechanical effects of the air in its cavities, partly to the derangement of the respiratory function, and partly to the circulation of blood

[1] Opus. cit., p. 81.

mixed with air in the capillary vessels of the systemic circulation being unable to maintain the nutrition, and the vitality of the tissues, especially of such important organs as the brain, the spinal cord, the lungs, and the heart. We do not think it necessary to enter into any critical examination of the arguments advanced by the supporters of these different explanations of the cause of death, as the account which we have already given of the appearances observed after death, and the symptoms which precede it, but more especially the former, enables us to select the true one. As in almost all cases in dogs, in the majority of rabbits, and in individuals of the human species, and in a considerable number of horses and sheep killed either by the forced or the spontaneous entrance of air into the veins, no air was found in the left side of the heart, or in the arterial system, it is perfectly obvious that in all these cases the circulation of air in the arteries of the brain, and in the medulla oblongata, could not be the cause of death: for a thing which did not exist could not act. The theories of Bichat and Sir Charles Bell cannot therefore explain the cause of death in all or even the greater number of cases, and there is no evidence in their favour, even in those cases where air was found in the left side of the heart and in the arterial system, for in all these the right side of the heart was on an average not less distended with air or frothy blood, and the death was not more rapid than when the air was entirely confined to the right side of the heart and the venous system. If, then, the rapid and great distention of the right side of the heart, and the arrestment of the circulation through the lungs, be sufficient to account for the sudden death, and of this there can be no doubt, and if, as we have seen, these conditions be found constantly present in all experiments worthy of confidence, it is quite unphilosophical to call in the agency of other causes to account for this effect, when we are already in possession

of one sufficient for the purpose, and admitting of general application, the more especially if the others are only occasionally present, there being, moreover, no facts to prove that even when present, they influence the results. We do not deny that the circulation of frothy blood in the arteries of the brain may produce death, and we can conceive some cases where the frothy blood may pass more readily through the capillaries of the lungs, as sometimes occurs in the horse, where the functions of the central organs of the nervous system might be seriously affected by the circulation of frothy blood in their capillary arteries. It is especially worthy of remark, however, that in Amussat's experiments the period of death did not appear to be hastened by the presence of air in the arterial system, and in those cases in which the air was confined to the right side of the heart and the venous system, the death was on an average equally rapid, and attended by the same symptoms as in those in which the air had reached the left side of the heart and the arterial system. Besides, as far as we can judge from the result of experiments, the death would not be so rapid when the air enters the venous system, if it acted in this manner, and the symptoms preceding it would be somewhat different. When Nysten injected quantities of air into the carotids, the animals were seized with all the symptoms of apoplexy, became comatose, and did not die until the lapse of some hours. The writer of this has elsewhere stated[1] that he injected half an ounce (by measure) of air along the carotid artery towards the brain, and that this was followed by strong convulsions of the limbs and trunk, succeeded by coma, which continued for at least four hours before the animal died. He also adds, "I

[1] Experimental Investigation into the Functions of Eighth pair of Nerves, Edin. Med. and Surgical Journal, No. 139, Expt. 28. [Vide p. 195 of this volume.]

have repeated the experiment of blowing air along one of the carotids, or vertebrals, towards the brain, and always found that it produced convulsions and coma, lasting for some time before death. The quantity of air injected was from half an ounce to an ounce, and even more. The mode of death in such cases is very different from what I observed when air was injected *rapidly*, and in considerable quantity, into a vein leading directly to the heart, for it then kills by mechanically arresting the movements of the right side of the heart." Lest any one should be led into error by the application of the term syncope to the sudden cessation of the sensorial and respiratory functions, after a certain quantity of air has entered a vein in the human species, we may mention that it is, more correctly speaking, a sudden case of asphyxia arising from the rapid arrestment of the circulation of the blood. Amussat's patient, when describing the difference between his feeling in this state and in common syncope, for he had experienced both within a short time, said, that in the former, "le trouble ne vient pas d'en haut, il vient d'en bas." The supposition that when air enters the venous system in large quantities, it produces emphysema of the lungs, has been rendered completely untenable by the observations of Cormack and Amussat. We believe, then, that it cannot be doubted that when an animal dies from the immediate effects of the entrance of air into the venous system, this is caused by the arrestment of the circulation through the right side of the heart and the lungs. Two circumstances produce the sudden distention of the right side of the heart, and these are, 1*st*, The mechanical impediments to the circulation of the blood through the right side of the heart from the presence of a large quantity of an elastic fluid in its cavities; and, 2*dly*, The great difficulty, and in many cases impossibility of transmitting frothy blood through the capillaries of the lungs. The operation of the latter cause is

illustrated by the experiments of Magendie and Dr. Cormack upon the effects of injecting air into the mesenteric veins, in which it was shown that the air could not pass through the capillaries of the vena portarum ; and also by an experiment of Mr. Erichsen, who found that a pressure of from one and a-half to two inches of mercury, was sufficient to propel beat bullock's blood through the pulmonary capillaries of a dog recently dead, while it required a pressure of from three to three and a-half inches of mercury to drive the same blood when mixed with air through these vessels. In those cases in which the air enters rapidly and in great quantities, the distention of the right side of the heart may be suddenly produced, and be the primary cause of death, while, in those cases where it enters more slowly, the distention of the right side of the heart may, to a considerable extent, be a secondary effect, and be produced chiefly by the difficulty it experiences in propelling the frothy blood through the lungs. By the agency of these two causes, the circulation of the blood is suddenly brought to a stand, and all the other vital functions are consequently rapidly suspended.

6. *Spontaneous entrance of Air into the Veins.*—The experiments of Amussat have demonstrated that when an opening is made into certain veins in the domesticated animals, the atmospheric air may enter in quantities sufficient to produce death in a few minutes. This result in the normal condition of the part operated on, occurred only in the veins where the respiratory flux and reflux of the blood were observed. These veins are placed in the anterior part of the neck, the superior part of the chest, and in the axilla, and consist of the jugulars, the subclavians, the axillary veins, and some of their larger branches at their termination near the summit of the chest. Amussat also points out that this dangerous space, " l'espace dangereuse," may be circumscribed by two semi-elliptical lines passing

from one axilla to the other,—the one above and the other below the clavicles.[1] This dangerous region may be extended by certain physical conditions of the veins, but before we can understand how these operate, it is necessary that we have a distinct knowledge of the causes which produce or facilitate the entrance of air into a vein, when an opening has been made in it. During expiration, when all the parts within the thorax are compressed, the large veins leading to it become turgid with blood, while, during inspiration, the compression of the parts in the interior of the thorax being removed, the atmospheric pressure is for a time less on the veins within the chest than on those outside it, the blood in the distended veins rushes onward towards the heart, and these veins consequently become collapsed. This respiratory flux and reflux is more or less extended, as the respiratory muscular movements are more or less forcible and prolonged. During inspiration, therefore, the blood is drawn into the interior of the thorax from the large veins in its immediate neighbourhood, and this sucking influence, or *vis a fronte*, would have extended to a greater distance from the chest, had the veins, like the trachea, been provided with walls capable of bearing off the atmospheric pressure. When we tie a tube like a vein upon the nozzle of a common injecting syringe, and attempt to draw water through it by pulling up the piston, we perceive that the part of the tube immediately in front of the nozzle of the syringe alone becomes empty, and that the water beyond this is prevented from passing onwards by the atmospheric pressure forcing to-

[1] [Bernard, in his attempts to reach the roots of the spinal accessory nerve in dogs by trephining the occipital bone (Archives Générales de Médecine 4e. série, tom. v. p. 54. 1844,) found that air freely entered the venous sinuses and distended the right side of the heart, so that he believes that the animals died from the entrance of air into the vascular system, and not from hæmorrhage. This might also occur in the human species.]

gether the walls of the tube. That air would be drawn through an opening in one of the large veins leading to the chest placed at points beyond the limits of this respiratory flux and reflux, were their coats capable of bearing off the atmospheric pressure, is proved by the experiments of Magendie and Amussat, who introduced tubes with resisting walls into an opening in the jugular, at some distance above this flux and reflux, and found that when they were passed sufficiently far in the direction of the heart, the air readily entered the vein during each expiratory movement. Wattmann, (p. 34,) as some other authors have done, conjoins the sucking influence of the right side of the heart to that of the respiratory movements in drawing air into the veins, and he attributes, like some of the older and even more modern physiologists, the relaxation or diastole of the heart, to a vital power of expansion. It would be easy, however, to show that the arrangement of the muscular fibres of the heart, and the phenomena attending muscular contractility, are at variance with the opinion that the relaxation of the heart is dependent upon any other than mechanical causes. A regurgitation, to a small extent, along the large veins entering the heart, is observed during each contraction of the right auricle, and this becomes more considerable when there is any impediment to the free passage of the blood through the right ventricle, or when the blood becomes stagnated in the veins, but the sucking influence of the right auricle must be so trifling that it can give very little aid to the sucking influence of the lungs. Amussat, in three experiments, found that when an opening was made into one side of the chest, the passage of the air into the opening in the vein ceased, and was immediately renewed when the opening in the chest was obstructed. Bérard thinks that this sucking influence is aided even in the normal state of the parts, by the mode in which the veins of the neck are

connected to the fasciæ; and Sir Charles Bell has maintained, and in our opinion on good grounds, that the entrance of air into an opened vein at the lower and lateral parts of the neck is much facilitated by the mechanical expansion of the veins during powerful contractions of the muscles of the neck, and the elevation of the shoulders in forced inspiration, a circumstance very likely to occur during an operation. From these facts it must be perfectly obvious, that when an opening is made into a vein in the proximity of the chest in the human species, atmospheric air may enter and pass on with the blood to the heart, for the same mechanical causes which effect this in the domesticated animals, must operate with equal force in man. It is also apparent that the circumstances which prevent the walls of the veins from being pressed together by the weight of the atmosphere, must extend the limits of this dangerous region. The most common of these are the following:—The pulling and stretching of healthy veins by the manipulations of the operator or his assistants, or by the position of the muscles; the outer surface of the vein being surrounded by, and adhering to morbid deposits and growths; the thickening of the coats of the veins or the surrounding areolar tissue by inflammation or other causes; and the increased size and width of the smaller veins, as occurs in some cases of what have been termed aneurism by anastomosis. Wattmann, in enumerating various circumstances which enable us to explain the discrepancies in the accounts given of different cases of the accidental entrance of air, mentions among others the following:—1*st*, "The erect position of the patient during the operation; for the more the part operated on is placed higher than the heart, so much the less are the veins distended with blood, and so much the more readily will air enter them in greater quantity when opened, if at the same time their walls are prevented from being forced together by the at-

mospheric pressure. *2d,* The partial division of the vein, permitting the air to pass on mixed with the blood which still continues to flow onwards to the heart, as I myself observed in the human species, and which Cormack and Mercier have also made mention of. *3d,* The complete division of the vein allowing the air to enter in those cases where the central or proximal end gaps, or is kept expanded, and in such a case is Cormack's observation correct, that air unmixed with blood will rush on to the heart."

When air enters spontaneously by an opening in a vein, two sounds may be heard—one depending upon the passage of air through the opening made in the vein—the other synchronous with the contractions of the heart, is caused by the intermixture of the blood and air in that organ. This second sound must, from its nature, be also present in those cases in which the air is forced into the vein, while the first can only be heard when it enters spontaneously. The first sound ceases immediately on obstructing the opening in the vein, while the second is not affected by this. Amussat describes the first sound as being faint (*un bruit peu fort,*) in rabbits; as a lapping sound (*bruit de lapement,*) in three out of four sheep, in the greater number of dogs, and much more rarely in the horse; as a gurgling or *glou glou* sound (*bruit de gargouillement ou glou glou,*) in five out of seventeen horses, and in a very few cases in dogs. In one sheep he heard a clicking sound; in six of the horses he does not describe it specifically, but in four calls it a very loud sound, (*bruit très fort,*) and in two a peculiar sound (*bruit particulier.*) In thirty-four cases of the accidental entrance of air into the veins in the human species, collected by Wattmann, he says, (p. 47,) it is described as a hissing sound in thirteen;[1] as a gurgling or bubbling

[1] Cases of Dupuytren, three by himself, Roux, Ulrich, Barlow, Pierre and Coudougnés, Delaporte, Malgaigne, Velpeau, Mirault, and Toulmouche.

sound in five;[1] a sound of aspiration (*bruit d'aspiration*) in two;[2] and a suction or strong sniffling sound in one;[3] as a whizzing sound (*sausendes Geräusch*) in one;[4] as a jerking sound (*bruit saccadé*) in one;[5] as a peculiar sound (*eigenthümliches*) in one;[6] a deep sound (*tiefes*) in one;[7] as a rattling sound (*rasselndes*) in one;[8] and in eight the sound was either not present or not heard.[9] These differences in the sound must depend chiefly upon the size of the opening in the vein, the force with which the air enters, and upon the fact whether or not blood be flowing out at the opening while the air is entering. Wattmann has pointed out (p. 45) that blood does not necessarily, as some seem to believe, flow from a cut vein. When there is no impediment to the onward passage of the blood, and when the vein is not quite full, as generally occurs when the part in which the vein runs is in a vertical position and above the level of the heart, and when less than half the circumference of the vein is cut through, the blood may not flow out at the opening in the vein.

The second sound, as we have already mentioned, is synchronous with the systole of the heart, and varies in character. Nysten described it as resembling the sound produced

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[1] Castara, Warren in two cases, Bégin, Pellis.

[2] Clémot in two cases. [3] Delpech.

[4] Rigaud. Rigaud, however, really describes the sound in these words, "J'entendis alors un bruit d'aspiration de l'air par trois fois différentes."

[5] Amussat.

[6] Mott. In the *Gazette Médicale* 1831, to which Wattmann refers for an account of this case, it is described not as a peculiar sound, but un bruit de gargouillement.

[7] Beauchêne. Piédagnel, in his account of this case, says, that the sound resembled the air entering a small opening in the chest of a living animal.

[8] Mayor. Mayor himself says, "bruit terrible de ronflement."

[9] One of these eight was a case of suicide, and there was no opportunity, even had a sound been present, of hearing it. Wattmann, as we have seen from the above notes, has not classified quite accurately these cases according to the sounds heard. We may also further add, that in Malgaigne's case, it was a gurgling and not a hissing sound.

by beating up the white of an egg with water (*en battant ensemble du blanc d'oeuf avec de l'eau.*) Amussat in applying the stethoscope over the region of the heart in some of his experiments heard this sound, which varied in different cases, for he describes it as a *bruit de souffle*, *bruit humide*, *bruit de gargouillement*, *bruit de rape*, *bruit de coassement de grenouille.* This second sound has also been heard in some of the cases of the accidental entrance of air in the human species. In that of Delaporte it is stated that " un bruit de sifflement se fit entendre à tous les assistans *avec un gargouillement dans la poitrine.* The seven cases in which air entered the veins in the human species during operations, without any sound being remarked, cannot, says Wattmann, " diminish the certainty and importance of these sounds as signs of the entrance of air into the veins, for there are circumstances sufficient to explain why these sounds may not be heard even when present. To these belong the unexpected occurrence of this phenomenon, and its being overlooked when its signification was not yet known ; and also a wider opening in the vein. I plainly remarked that the hissing sound ceased, as I separated the edges of a wound in the vein about the size of the opening generally made in venesection, wider from each other," (p. 52.) The characteristic phenomena attending the entrance of air into the veins in the human species, are, according to Wattmann, the following :—" Want of bleeding from the opened vein in the erect position ; in the horizontal position proportionally little hæmorrhage ; a loud sound occurring while making an incision ; the moist sound (second sound) may not on account of the threatened danger be observed ; rapid appearance of constitutional symptoms, such as paleness, trembling, and deep syncope, sometimes anxiety and cries, convulsions very seldom and of short duration ; speedy death ; and on inspection of the body, air is found in the blood-vessels, more especially in the

central organs of the circulation." (p. 65.) The observing the first sound is evidently a point of very great importance, as it indicates to the surgeon that very serious consequences are likely to happen, which may, however, be still entirely, or in a great measure prevented, by active and judicious management.

7. *Remedial measures to be employed against the consequences of the entrance of Air into the Veins, and for their prevention.*—These questions are treated of at great length by Wattmann. (p. 80.) After pointing out the methods of ascertaining whether when a sound of air rushing through an opening is heard, while the surgeon is operating on the lower and lateral part of the neck, it comes from an opened vein or the sac of the pleura, he goes on to state that when this comes from an opened vein, the surgeon has neither time nor opportunity to make any examination of the place whence it proceeds, for the patient is liable without loss of blood suddenly to fall into a syncope, with or without convulsions preceding, and is placed in the greatest danger. "It is enough to know, that *immediately after hearing the sound*, the opening from whence it proceeds should be shut by slight pressure of the point of the finger. Then draw the finger along the course of the vein toward the heart, sufficiently far to expose the opening to view. The sound ceases on the application of the compressing finger, and on exposing the opening in the vein, we observe blood flowing from it, or we see through the opening appearances which indicate that it is placed in a vein, so that now no doubt can be entertained both regarding the wound in the vein and the entrance of the air. In many of the recorded cases of wounds of the veins in the dangerous region, death has occurred with surprising quickness; while in those cases where the entrance of air into the veins was soon arrested, the individuals were saved, and the same thing has been observed in the lower animals subjected to

experiment. In this dangerous region, nature has no means of preventing the entrance of air into the veins, but beyond its limit the walls of the veins would be forced against each other by the atmospheric pressure, and the passage of air along the veins prevented. This hint given by nature can also advantage art, in bringing aid in these cases where nature cannot possibly help herself. Since the entrance of air alone through a wound in a vein will occasion danger of life, and even sudden death, so can the indication of cure be no other than to *prevent the entrance of air into the veins as soon as the conditions for this are established, as rapidly and permanently as possible.* In so far as the conditions for the passage of air through the opening in the vein are concerned, this question also presents itself, how *to avoid wounding a vein in this dangerous region, with the view of escaping this risk.* The one object is therapeutical, the other is prophylactic. The means which have been brought into use for effecting the therapeutical object are partly founded upon the remedial means applied in ordinary cases of wounded veins in the human species, partly upon the results obtained in experiments upon animals, and partly upon the different ideas entertained of the nature of the deleterious agency of the air, and the ends which it is wished to obtain. The purposes wished to be attained have been—preventing the passage of air into the vein; removal of the air which has entered the vascular system; the withdrawing of a certain quantity of blood; and recovery from the syncope: many, as Magendie, Nysten, Bouley, Mercier, Denot, wished to attain a single one of these ends, others more of them in combination." Wattmann next proceeds to give a short account of the methods by which the various authors who have written on the subjects have attempted to attain the ends mentioned above. Under the section (p. 84) entitled *first method of obtaining the first purpose,* viz., preventing the

entrance of air into the veins, he mentions the different recommendations to effect this, viz., limiting the extent of the respiratory movements, the application of external pressure to the chest and abdomen by bandages and other means. Under the section entitled *second method of obtaining the first purpose*, he gives the different directions which have been laid down for preventing the entrance of air into the veins, without restraining the respiratory movements. This method consists in making pressure upon the opened vein: its great importance was first illustrated, the rules for its proper application explained, and its first practical application made in man by Wattmann himself. The *second purpose*, viz., The withdrawing of the air which has entered the venous system, has been attempted by external pressure upon the chest and abdomen during respiration, so as to force out part of the frothy blood through the opening in the vein; by opening a large vein at the summit of the chest and as near the heart as possible, to allow part of the frothy blood which regurgitates from the right ventricle at each contraction, when the blood ceases to pass freely through the lungs, and it becomes distended with blood; and by withdrawing the air and frothy blood from the heart by sucking it through a syringe, or through a tube, introduced into the opening in the vein, and carried downwards towards the heart. *The third purpose*, viz., that of withdrawing a quantity of blood, has been recommended by those who believe that the means adopted to withdraw the air are also useful in unloading the venous system and right side of the heart of their blood, and thus facilitating the efforts of the right ventricle to propel the blood through the lungs. For effecting the *fourth purpose*, viz., recovering from the syncope, all the means employed in syncope from other causes, have been used and recommended, such as placing the head in a horizontal or depending position, dashing cold water on the face, the appli-

cation of stimulating vapours to the nostrils, &c. Artificial respiration has been also employed in a few of the cases, and Warren recommended that it should be kept up for half an hour or an hour. Mercier advises that pressure be applied to the abdominal aorta and axillary arteries, to send as large a quantity of blood as possible towards the brain, and he states that he succeeded in recovering a dog by this means.

Since the possibility of the entrance of air into a wounded vein in those parts included by Amussat, in his "*espace dangereuse*" cannot be doubted, the surgeon ought to use every precaution while operating in this region, and even in parts beyond it,—if the coats of the veins are likely to be capable of resisting the atmospheric pressure,—to avoid the wounding of a large vein. It may be advisable to follow in some cases the advice given by Dupuytren, to remove the tumour in different portions, when this is likely to facilitate its separation from the large veins in its neighbourhood. As in some cases the large veins may be involved in, or displaced by tumours, and are liable to be wounded, notwithstanding every precaution has been taken to avoid this, the surgeon should keep steadily in mind the conditions which favour the entrance of air into the veins, to provide against their occurrence, and to counteract their influence. All stretching of the parts, either by the hands of the surgeon or his assistants, likely to keep the veins expanded, should, if possible, be guarded against. As it is during inspiration that the air is drawn along the veins, the surgeon would act prudently in suspending his incisions during its performance, when dissecting in the immediate neighbourhood of large veins. The application of bandages to the chest and abdomen cannot hinder the air from passing along a vein, but it may limit the extent of the respiratory movements, and in this way prevent the air from entering so rapidly, and

in such great quantity, as when the inspirations are more full and forcible. The compression of the main trunk or trunks of a vein between the heart and the part operated on, has been recommended. This plan might be advantageously adopted in some cases where the distention of the veins with blood is not apt to embarrass the operator; while in others, there may not be sufficient space left on the proximal side of the part operated on, to permit it to be put in practice. If during an operation a sound indicating the entrance of air into a vein be heard, the surgeon should instantly place the point of his finger upon the place from whence the sound proceeds, and arrest the passage of any additional quantity of air into the vascular system. Wattmann appears to think that by attending to this, the surgeon can prevent a fatal quantity of air from entering the vein: and in all probability he saved in this manner the four patients to whom this accident occurred in his own practice. It is also probable that one of Warren's cases, one of Clémot's, the cases of Malgaigne, of Rigaud, of Bégin, of Velpeau, of Amussat, and of Mayor, were saved by the immediate compression of the wounded vein. This, of course, is only a temporary method of arresting the entrance of air into the veins, and can only be employed until some permanent method be adopted. This latter may be effected by bringing the edges of the external wound together, and applying pressure if necessary; or by a ligature tied around the circumference of the vein; or by a ligature applied so as to bring the edges of the wound in the vein together without obliterating its calibre. The first of these methods, viz., by bringing the edges of the external wound together, ought always to be employed when practicable. The third method, viz., by bringing the edges of the wound in the vein together (seitliche Verbindung), without obliterating the calibre of the vessel at the point tied, was first practised by Wattmann, and is strongly recommended by him in

those cases where the vein wounded brings its blood from an organ whose functions are easily disturbed by derangement of the circulation of blood, such as the brain, if the wound in the vein be not too extensive, and has not implicated too large a portion of the circumference of the vessel to permit of this. The mode in which he first applied this method to practice, is detailed in the first case extracted from his work, given in the Appendix to this article. In this case he used two pairs of shutting forceps, and he also recommends, (p. 101,) the use of a single pair of broad shutting forceps, (termed *Kern's Sperrpinzette,*) with the free ends of their two limbs rounded, so that when shut they resemble the chisel-shaped end of the handle of a scalpel. In these cases where the wounded vein is in the neighbourhood of a bone, and lies so deep that the application of the ligature is difficult, as in the subclavian vein, he recommends the use of two small wooden shutting forceps, of which he gives a lithographic representation, (p. 105.) These are to be applied upon the vein so as to bring the edges of the wound in contact, and left there sufficiently secured until the usual inflammatory effusions have taken place, and they be loosened, like the ligature, by the ulcerative process. Though there appears to be little doubt that rapidly compressing the wounded vein by the finger, as soon as the sound indicating the entrance of air into the veins has been heard, is sufficient, in most cases, to prevent the entrance of a quantity of air capable of killing the patient; yet in some cases the sound may either not be heard, or the opening in the vein may be so large that no sound is present, or a larger quantity of air may have entered the vein before the surgeon is fairly aware of the nature of the accident; and he is now called upon to consider what means may be adopted to remove part of the air from the vascular system. One of these means, first recommended by Magendie, consists in

passing a tube along the interior of the wounded vein towards the heart, and withdrawing by suction with the mouth, or by a syringe, part of the frothy blood. Amussat, in some experiments, did not find this procedure so successful as he had anticipated. Nysten states that he recovered two dogs by forcing the frothy blood from the heart through an opening in the jugular vein by applying pressure upon the chest. Amussat also infers from his experiments that pressure upon the chest and abdomen, during expiration, favours the exit of air through the opening in the vein, and aids the heart in disembarrassing itself of the air which distends its right cavities. Dr. Cormack states, that on opening one of the large veins, in some experiments, near the upper part of the chest, the blood regurgitated from the right side of the heart, and its propelling power increased. These results are in conformity with, and explained by experiments made by the author,[1] who found that when the cavities of the right side of the heart became much distended with blood, the contractions of the stretched muscular fibres were much enfeebled, or even arrested; and that if the distending cause was removed within a short time after its occurrence, their contractions may be much increased in strength, or even renewed, after being altogether arrested. He also ascertained that in dogs the opening of the external jugular vein, when the circulation through the lungs was much impeded, and the right side of the heart distended with blood, permitted the blood to regurgitate from the cavities of the right side of the heart. In employing these various methods of emptying the right side of the heart of part of its blood, great care should be taken that no additional air enter the veins; and these precautions are especially necessary during the muscular movements of inspiration. It must be admitted, that

[1] Edinburgh Med. and Surg. Journal, No. 127, or p. 51 of this Volume.

though these different methods of freeing the vascular system of part of the contained air, in some cases materially assist the attempts to renew the circulation through the lungs, yet we can readily understand how they should fail in other cases, when the pulmonary artery and its branches have become filled with frothy blood. The active application of those remedies which call forth the excito-motory movements of respiration, may be of great advantage in favouring the circulation through the lungs; and if these fail in keeping up, or renewing the respiratory muscular acts, the surgeon may have recourse to artificial respiration. The doubtful success of all therapeutical agents, when a large quantity of air has entered the veins, ought to make the surgeon the more anxious in the application of prophylactic measures.

8. *Cases of accidental entrance of Air into the Vascular System in the human species.*—Our space will not permit us to enter upon any critical examination of the different alleged cases of the accidental entrance of air into the vascular system in the human species; and our remarks on this head will be chiefly of a general character. Velpeau has objected to the greater part of the cases with which he was acquainted, that either the symptoms during life, and the appearances after death, did not correspond with those observed in experiments upon the lower animals, where the cause of death could not be doubted; or they were imperfectly reported, or rested on hear-say evidence. Velpeau admits[1] that the introduction of air into the veins was *probable* in the cases of Bégin, Malgaigne, Mirault, Warren, Barlow, Delaporte, one of Clémot's, the first related by Roux, and in his own. He maintains that there is *no proof* that this occurred in the cases of Toulmouche, Mott, the second and third of Clémot, those of Rigaud, Dubourg,

[1] Opus. cit., p. 22.

Maugeis, and Amussat; that the occurrence of this accident was *extremely probable* in the cases of Delpech and Ulrich; and *almost certain* in the cases of Dupuytren, Castara, and Goulard. Amussat[1] divides all the cases of this kind with which he was acquainted into four groups. In the first, he places those cases which he believes to be incontestably of this nature, where the peculiar sound, the symptoms, the death, and the autopsy, resembled the results of experiments upon animals. These are nine in number, and include the cases of Beauchêne, Dupuytren (1822), Delpech (1830), Castara, Roux (1832), Ulrich, Roux (1836), Clémot (1830), and of Putegnat. In the second group he arranges those cases analogous to the preceding, where the autopsy was alone wanting to make them equally incontestable with those in the first group. These are six in number, and include the cases of Mirault, Warren (1833), Goulard, Barlow (1831), Duportail, and one communicated to him by a celebrated surgeon whose name is not mentioned. In the third group he places the cases of recovery. These are twelve in number, and include his own, the cases of Warren (1833), two cases of Clémot (1830), Mott, Mussey, Toulmouche, Delaporte, Malgaigne, Rigaud, Bégin, and Mayor. In the fourth group he places the doubtful cases. These he divides into two series—into those published since the possibility of the occurrence of this accident was known, and into those published anterior to this, but which have some analogy with the preceding. The first of these series includes three cases—those of Velpeau, Maugeis, and Barlow, published in 1831. The second series includes four cases—those of Bonnefoy, Briot, and two cases of Pelletan. The fifth group includes cases of the accidental entrance of air from wounds made in the dangerous region in attempts at suicide, and contains two

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[1] Opus cit., p. 95.

cases, those of Dr. Handyside and Pellis. Amussat, along with Velpeau, evidently rejects entirely—for he makes no mention of them—the alleged cases of Graefe, Klein, Sir A. Cooper, Lodge, and Dubourg; and Wattmann, in addition to the above, excludes altogether, and, in our opinion, very properly, the cases of Duportail, Putegnat, Maugeis, the first case of Barlow published in 1831, and the whole of the four cases contained in the second series of Amussat's fourth group. Had Amussat been acquainted with Wattmann's cases he would probably have ranged them in his second group. Wattmann's list of cases of the accidental entrance of air in the human species, which he thinks worthy of credit, contains thirty-five examples, and of these two were cases of suicide. This list includes his own four cases, and he has thrown it into the form of a table, found at the end of the volume, with different columns, showing in each case the veins wounded, the nature of the sound heard, the result, the period of death, &c.

After a careful perusal of the original details of the different cases retained in Wattmann's list, we feel inclined to purge it still further; but this would lead us into a discussion of the evidence on which the unsatisfactory cases rest, which our space would not permit. The greater part of these are, however, undoubted examples of the accidental entrance of air into the venous system, and are quite sufficient to illustrate the importance of this subject to the practical surgeon.

In the thirty-three cases of the accidental entrance of air into the veins, during operations on the human species, which Wattmann considers worthy of credit, it entered by the following veins:—In seven it entered by the internal jugular vein, viz., in the cases of Beauchêne, Wattmann, Roux, Ulrich, Bégin, Mirault, Velpeau. In two cases, those of Mussey, and one of Clémot's, it entered by the subclavian. In three cases, those of Goulard, and two of

Wattmann's, it entered by the axillary. In one of Warren's cases, and in Castara's case, it entered by the subscapular vein. In one of Dupuytren's cases, and in one of Clémot's, it entered by one of the veins in the axilla, but the particular veins are not mentioned. In five cases, those of Pièrre and Coudougnés, Rigaud, Malgaigne, by a surgeon not named, and one of Wattmann's cases, it entered by the external jugular vein. In one of Warren's cases, it entered by a vein passing between the internal and external jugular veins. In one of Dupuytren's cases, in the cases of Barlow, Mayor, and Delaporte, it entered by other veins in the neck. In a case by Dr. Stevens, referred to by Wattmann, it is said to have entered by the jugular vein, but whether internal or external is not stated.[1] In the case of Mott, it entered the facial vein in extirpating a diseased parotid gland. In Delpech's case, and in one of Roux's, it entered the veins around the shoulder-joints, during amputation. In Delpech's case, the veins were much enlarged and dilated. In Amussat's case, it entered one of the pectoral veins below the clavicle, while he was extirpating a huge tumour of the breast. And in the case of Toulmouche, and in one of Clémot's, it entered by some of the veins wounded in extirpating diseased mammæ. From these cases, it would appear that in a considerable number of instances of accidental entrance of air into the veins in the human species, the air entered by veins placed beyond the *espace dangereuse*, but in all these it is probable that the coats of the veins were rendered capable of bearing off the atmospheric pressure, or what has been termed *canalised*, by the effects of previous disease.

The medico-legal bearings of the subject.—Wattmann, in

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[1] Wattmann refers to Warren's Surgical Observations on Tumours, 1837, p. 182, for this case. The copy of Warren's work before us bears the date of 1839. At p. 259 he says, that Dr. Stevens mentioned a case of the accidental entrance of air, but he does not give any details.

the third section of his work (p. 123) examines them at some length, and points out how far the knowledge of them will enable us to throw light upon the questions—whether a wound in the neck of a person found dead has been inflicted during life or after death? and if the former; whether it had been inflicted by his own hands, or by those of some other individual? The evidences of a wound having been inflicted during life, involving a large blood-vessel, are considered to be loss of a considerable quantity of blood, infiltration of the cellular tissue in the immediate neighbourhood of the wound with blood, or some traces of inflammation in the wound. The absence of these signs are supposed to justify the conclusion that the wound has been inflicted after death. However true this may be in other parts of the body, it does not admit of general application in wounds of the neck, for it is very possible that when one of the large veins of the neck is wounded, the atmospheric air may enter the venous system in quantities sufficient to kill, before any great quantity of blood has escaped from the wounded vein. The presence of a large quantity of frothy blood in the right side of the heart would not only prove that the wound had been inflicted during life,—for air will not enter the venous system after the respiration has ceased,—but will also explain how death had occurred, notwithstanding the small loss of blood. Wattmann gives the details of the case of a woman found dead in bed, with several incised wounds in the neck, by two of which the internal jugular vein was opened at two places. It was evident from the extent of hæmorrhage and other circumstances, that the wounds had been inflicted during life: and Wattmann endeavours to prove that it was not a case of suicide but one of murder, for not only the appearance of the wounds showed that they were inflicted by another person, but it would be impossible for a person to make upon himself these two different incisions into the in-

ternal jugular vein, seeing that the air would rush in when the first was made, and induce sudden loss of consciousness. He quotes the two cases of Drs. Handyside and Pellis, as the only two examples published of the entrance of air into the veins of the neck by suicidal wounds. We feel quite satisfied that Dr. Handyside misinterpreted the appearances he observed in his case, when he attributed the death partly, if not chiefly, to the entrance of air into the veins; and no one can read Dr. Cormack's critical examination of it[1] without arriving at the same conclusion. The case of Pellis rests upon very different evidence; for here the *glou-glou* sound indicating the passage of air into the vein was not only heard, but the heart, when removed from the body, after all the large veins entering it had been tied, floated in water, and when the right ventricle was punctured under water, a great number of bubbles of air were disengaged.

[A translation of Wattmann's Cases of the Accidental Entrance of Air into the Venous System, during operations on the Human Species, with Remarks, is appended to this review, as originally printed in the Monthly Journal of Medical Science.

Olivier, in the article *Air*, in the Dictionnaire de Médecine, p. 73, Paris, 1833, refers to some experiments by Legallois, recorded in the Journal Hebd. de Méd., tom. iii., p. 183, et suiv. 1829, from which it appears that the spontaneous introduction of air into the veins may take place under circumstances besides those where these vessels happen to have been accidentally opened by an instrument. This dexterous experimenter had observed, on three different occasions, the air penetrate into the vena cava inferior

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[1] Cormack.—Remarks on a Case of Suicide, published by Dr. P. D. Handyside. 8vo. Edinburgh, 1838.

and the heart by the uterine veins, and cause instantaneous death in female animals on which he had been studying the effects of abstinence and the loss of blood in gestation. "Is it," asks Olivier, "to a cause of this kind that we ought to attribute the sudden and unexpected death in females lately delivered, and where the autopsy disclosed nothing which could account for such a catastrophe? This is," he continues, "a point in pathology which it appears to me important to direct attention to here, and which merits so much the more to be examined by practitioners as the examples of which it treats are not very rare." Professor Simpson has kindly furnished me with the following observations on this subject.

"Several years ago I saw a case of death a few hours after delivery, which first gave me the idea that death may sometimes occur from the introduction of air into the uterine veins. The patient was an inmate of the Lying-in-Hospital, and delivered of twins. Dr. Ziegler was called to see her in consequence of some difficulty connected with the birth of the second child. Post-partum hæmorrhage, with alternate contractions and relaxations of the uterus supervened; and she seemed to rally very imperfectly from the effects of the flooding. I saw her in an hour or two afterwards. She had then a very weak and rapid, almost imperceptible, pulse, an extremely anxious countenance, and here and there was an evanescent scarlatinoid rush over the surface of the body. The body was opened a few hours after death, as we were anxious to know if there was air in the veins; and we were of course desirous not to incur the fallacy of air arising from decomposition. The abdominal contents were exposed under water. The uterine, hypogastric veins, and lower vena cava, were full of frothy blood, and the air bubbled up through the water when these tubes were opened. The larger veins of the extremities were in the same state. On examining different

works to find if the scarlatinoid rush observed in the above patient had been seen in any surgical cases in which air had been introduced into the veins, I found Professor Warren of Boston mentioning the appearance of a scarlet eruption in two cases which had come under his own personal notice. Supposing it to be a result of entrance of air, may it be owing to the introduced air directly mixing with and oxygenating the blood in the capillaries? Since the above case occurred I have been called to three or four other instances in which the same symptoms were present, viz., great depression after delivery, an almost imperceptible pulse, and an erratic scarlatinoid rush on the surface, and where the patients died within two or three days after delivery. In one case, where the death was more rapid, no red rush could be seen. The first was the only case where an autopsy was procured. Two or three of the cases had been considered as malignant scarlatina.

" As to the mechanism of the introduction of air in such cases, supposing that to be the cause, I think we can understand it, when we remember that the interior of the uterus after delivery, especially opposite the late seat of the placenta, is studded with venous orifices, and that if air does once become introduced into the uterine cavity, from relaxation of the walls of the organ, it will be liable to be *forced* into these orifices, and hence into the general venous circulation, provided the uterus in again contracting is unable to expel its contents through the os uteri."]

No. XXV.

REVIEW.[1]

On Disorders of the Cerebral Circulation, and on the Connexion between Affections of the Brain and Diseases of the Heart. By George Burrows, M.D. London, 1846. Pp. 220. With Illustrative Drawings.

(FROM LONDON AND EDINBURGH MONTHLY JOURNAL OF MEDICAL SCIENCE FOR AUGUST 1846.)

This work, as its title sets forth, embraces two distinct subjects—an examination of the derangements of the circulation within the cranium, and the connexion between the affections of the brain and diseases of the heart. The author has carefully and dispassionately considered the questions upon which he undertakes to enlighten us; and though we have the misfortune to differ from him in opinion on many points, especially in the first of these subjects, and entertain no doubt that several of his conclusions are by no means warranted by the premises, yet we are bound to state that this work, as a whole, reflects credit upon him, and exhibits considerable talent and much professional information. If, therefore, in any of the strictures we shall

[1] In writing the following Review my object was rather to show that the objections urged against the doctrine of Monro *Secundus* and Dr. Kelly, regarding the circulation within the cranium, were by no means so conclusive as seems pretty generally to be imagined, than to declare an unqualified adhesion to this doctrine, for I believe the subject requires farther investigation.

have occasion to make upon his views of the circulation within the cranium, we should criticise sharply particular statements, and should make use of any expressions which may appear depreciatory of his judgment or his reasoning powers, we beg our readers to attribute this to no want of personal respect, or to any intention of parading his short-comings, but to our desire to inculcate stricter habits of thought, and more accurate methods of investigation, than are commonly found among our professional brethren. In analyzing closely the matter contained in most treatises on practical medicine, facts and opinions are with difficulty separated from each other, and no satisfactory evidence can be found of the existence of truths upon which important inferences are founded.

The circulation within the cranium possesses several peculiarities, which not only excite the attention of the anatomist, but are constantly referred to by the physiologist and pathologist, in their discussions upon the functions and diseases of the important organ there contained, viz., the encephalon. The derangements of the circulation within the cranium are so often followed by such serious consequences, that it becomes an object of the highest practical importance to endeavour to ascertain the nature and cause of these derangements; and, of course, the first and indispensable step, in an investigation of this kind, is to obtain an accurate knowledge of the manner in which the circulation is carried on in the healthy state. There is no one organ in the body, upon the healthy or diseased states of which the medical man is so often required to decide, as the encephalon, and there is none upon which more serious errors are committed in this respect—errors not only involving the reputation of the practitioner, but what is of infinitely more importance, exercising an influence in medico-legal cases, upon the momentous consequences of innocence or guilt, of acquittal or punishment.

There are certain peculiarities of the circulation within the cranium, connected with the distribution and structure of the arterial and venous systems, which are passed over by our Author; and he proceeds at once to a close and searching examination of that peculiarity of the circulation within the cranium first pointed out by Monro Secundus, tested experimentally by Dr. Kellie of Leith, and ably illustrated by the late Dr. Abercrombie. The views adopted by these able men were—that the cranium forms a spherical bony case, capable of resisting the atmospheric pressure, as the only openings into it are the different foramina, through which the vessels, nerves, and spinal cord pass; that the encephalon and its membranes, the blood contained within the vessels, the serous fluid secreted from the inner surface of the arachnoid, and a small part of the cerebro-spinal fluid placed between the outer surface of the arachnoid and the pia mater, fill up completely the interior of the cranium; and that no part of these substances can be dislodged from the interior of the cranium without some equivalent taking its place. For example, since the walls of the cranium resist perfectly the effects of atmospheric pressure, not a drop of blood would flow out from it through the internal jugular veins, unless a corresponding quantity passed into it through the arteries, or some other fluid by another channel, or unless there was an expansible substance within the cranium to occupy the room of the fluid expelled (but there is none such); for the pressure of the external atmosphere, acting upon the soft parts which cover the jugular veins, would effectually prevent its escape. It is perfectly impossible to empty a jar, or any vessel similar to the cranium, with unyielding walls, filled completely with water, or any other fluid or solid material, without the atmospheric air or some other substance taking its place. This is a law in pneumatics so familiar to all, that it stands in need of no illustration. We believe that the same kind

of reasoning applies to the spinal canal; in fact, we may consider the spinal canal and cranium as forming one large cavity, which cannot be diminished by atmospheric pressure. Admitting that a quantity of the cerebro-spinal fluid can be forced from the spinal canal within the cranium, by an accumulation of that fluid within the spinal canal, or by an increase of the blood either in the spinal arteries or veins, a proportionate quantity of blood must be pressed out of the vessels within the cranium, and the result would be the same as if serum had been effused from the cerebral vessels themselves. Since the substance of the brain and its membranes are incompressible, at least by any force which may be exerted upon them by the heart; and as the blood contained within the vessels must also be incompressible by the same force,—it follows, that at every stroke of the heart, when a certain quantity of blood is driven into the interior of the cranium along the arteries, an equal quantity must be dislodged through the veins, provided that no change has, in the meantime, occurred in the quantity of the other parts placed within this cavity. We may here state, that, though fluids are not absolutely incompressible, yet it requires the weight of one atmosphere, or fifteen pounds on the square inch, to produce a diminution equal to 1-22,000th part of the whole. Now, this is so exceedingly small a change upon a mass equal in bulk to the brain, as not to be appreciable by our senses; and as we are not reasoning as mathematicians or natural philosophers upon 22-000th parts, but as physiologists and pathologists upon sensible quantities, we may fairly proceed upon the supposition that the action of the heart can produce no change upon the quantity of fluids within the cranium, for the heart, in its most violent contractions, cannot exert a pressure equal to one atmosphere, or, in other words, produce a diminution equal to a 1-22,000th part. Under ordinary circumstances, the pressure upon the inner surface of the blood-vessels

may be between 3 lbs. and 4 lbs. on the square inch; and this may perhaps be increased to 10 lbs. or 12 lbs. during a very violent exertion. Dr. Burrows argues, in opposition to this doctrine, as follows, in page 35:—"Atmospheric pressure is undoubtedly exerted on the blood in the vessels entering the cranium. This pressure, by a well-ascertained law in hydrostatics, must be transmitted in all directions through the fluid blood, and hence to the blood and other contents within the cranium. If, in the natural state of the parts, the brain is defended from atmospheric pressure, should we not expect to find the functions of that organ disturbed in some way when part of the walls of this sphere is wanting? But in children with open fontanelles, and in adults who have lost part of the bones of the cranium, we observe no peculiar disturbance of the functions of the brain from this gap in the walls of the imaginary sphere." We, for our part, would never expect any disturbance of the circulation within the cranium under the circumstances mentioned by Dr. Burrows, and we should have wished to learn the grounds on which he founds his expectations. Dr. B. correctly points out how the atmospheric pressure is exerted, through the blood entering the cranium, upon all parts of the brain, and the removal of a portion of its osseous case, so as to allow the external air to press upon its outer surface, would not alter the amount of pressure upon the whole brain; it would only alter the manner of its application. Dr. B. goes on to say:—"But, lastly, the effects of gravitation on the fluid contents of the cranium, and the effects of the cupping-glasses, which will draw blood from the vessels of the dura mater, causing ecchymosis there, assure us that the cranium is not a perfect sphere in the sense in which it has been supposed." We shall examine the effects of position on the fluid contents of the cranium in a subsequent part of our remarks on this subject; and with regard to the alleged effects of the cupping-glasses

upon the circulation in the vessels of the dura mater, we must express our entire disbelief. If this statement were true, the application of cupping-glasses over the scalp in meningitis would greatly aggravate the disease; but we do not rest our denial of its accuracy upon this ground.

Dr. Kellie[1] made several experiments on sheep and dogs to ascertain the state of the vessels within the cranium after a fatal hæmorrhage, and these were varied as much as possible, to avoid sources of fallacy. Some of these animals were bled to death by opening the carotids or femoral arteries, others by opening the jugular veins; in some, the carotids were first tied to diminish the quantity of blood sent to the brain, and the jugulars were then opened with the view of emptying the vessels of the brain to the greatest possible extent; while in others the jugulars were first secured to prevent as much as possible the return of the blood from the brain, and one of the carotids was then opened. The inference of these carefully performed experiments we give in his own words: "That we cannot, in fact, lessen to any considerable extent the quantity of blood within the cranium by arteriotomy or venesection; and that when, by profuse hæmorrhages destructive of life, we do succeed in draining the vessels within the head of any sensible portion of red blood, there is commonly found an equivalent to this spoliation in the increased circulation or effusion of serum, serving to maintain the plenitude of the cranium."[2] Dr. Kellie also adduced the results of experiments upon the effects of position, immediately after death, upon the quantity of blood within the cranium; and the appearances observed within the cranium after death from strangulation or hanging, in favour of the truth of the doctrine we are illustrating. With the view of obtaining

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[1] Medico-Chirurgical Transactions of Edinburgh, vol. i. 1824.

[2] Opus cit., p. 123.

more satisfactory evidence on this question, he first removed a portion of the unyielding walls of the cranium in some animals by means of a trephine, and then bled them to death, and the differences between the appearances of the vessels of the brain in these cases and in those where the cranium was left entire, were very great. One of the most remarkable of these differences was the shrunk appearance of the brain in those animals in which a portion of the skull was removed, and the air allowed to gravitate upon its outer surface. In describing the appearances of the parts within the cranium of the first of the three animals trephined, he says, "the brain was sensibly depressed below the cranium, and a space left, which was found capable of containing a tea-spoonful of water."

Dr. Burrows, in the work before us, endeavours to show that there is no such peculiarity in the circulation within the cranium as has been contended for by Drs. Monro, Abercrombie, and Kellie; and this he attempts to do by appealing to the result of some experiments, neither so varied nor so extensive as those of Kellie, performed by himself; to various anatomical facts; and to pathological and physiological observations, collected by himself and others. The following quotation (p. 33) contains a summary of the conclusions at which he has arrived:—

"(1.) It is maintained, that when hæmorrhage takes place from the general system, it does not affect the quantity of blood in the brain. The experiments I have performed lead me to the opposite conclusion.

"(2.) Posture of the body after death is said not to affect the quantity of blood within the head. My experiments show that posture has a most striking influence on the quantity of blood in the cerebral vessels.

"(3.) It has been attempted to prove, that when individuals die of asphyxia or apnœa, there is no excessive congestion of the cerebral vessels. Numerous observations show that, in the different kinds of death by apnœa, there is great congestion of the cerebral vessels, and that where it is absent, it may be accounted for on anatomical and physical principles.

"(4.) It has also been attempted to prove, by an algebraical equation, that if the quantity of blood be diminished in one system of cerebral vessels, it must be increased in the other vessels. In reply to this, I have shown that the results of experiments negative this conclusion. The error lies in

the false assumption of the elements of which the equation is formed. It is also clear that there may be variations in the quantity of blood in one set of cerebral vessels without affecting the condition of the others, because the quantity of extra-vascular serum in the cranium will accommodate itself to the varying states of the blood-vessels."

The first proposition, viz., "that when hæmorrhage takes place from the general system, it does not affect the quantity of blood in the brain," stated in the above quotation to be laid down by those who contend for the peculiarity in the circulation within the cranium which we are discussing, instead of being a correct embodiment of this part of the doctrine, *is an actual misrepresentation of it;* and in proof of this averment, we need only refer to the quotation given above from the papers of Dr. Kellie. It is there distinctly admitted, that we may succeed in draining the vessels within the cranium of part of their red blood, but he adds, "There is commonly found an equivalent to this spoliation in the increased circulation or effusion of serum serving to maintain the plenitude of the cranium." Besides, it is asserted by no one, as far as we are aware, that the quantity of blood in the *brain* may not undergo variation from slighter causes than extensive hæmorrhage; for, according to the supporters of this doctrine, *the relative quantities* of blood in the vessels of the encephalon, and in those external to it, may and do undergo *alteration.* The proposition, to be correctly stated, should have been in this form: "That when hæmorrhage takes place from the general system, it does not affect the quantity of *fluids* within the *cranium.*" With regard to the criticisms made by Dr. Burrows upon Dr. Kellie's experiments, and the result of his own, we may make the following remarks:—That the vessels within the cranium should appear to be less filled with blood, as observed by Dr. Kellie in some of his experiments, after the animals had been bled to death, than in those killed by a dose of prussic acid, is easily explained. It is a well known fact, that when an animal is bled to death, the

blood last drawn is more serous, or, in other words, has fewer red particles in it than in that first drawn, and when means are not taken to prevent the free circulation, and, consequently, the frequent renewal of the blood within the cranium during the time the animal is bleeding to death, the vessels of the brain must necessarily appear less injected after death in an animal that has been killed by hæmorrhage from a single artery, though they may actually contain the same amount of *fluid*. Some slight differences between the relative injection of the vessels of the encephalon in a sheep bled from the carotid, and one bled from the jugular vein, may be explained in the same manner. When the carotid artery has been opened, the quantity of blood sent to the brain must be diminished, and the quantity returning from it by the veins must also be diminished, and consequently, the blood within the cranium must be less frequently changed than when an animal is bled to death from the jugular, where there is nothing to diminish its free circulation through the vessels within the cranium, except what arises from the decreasing supply caused by the hæmorrhage. Besides, the blood flows somewhat more rapidly from the carotid than from the jugular, and the circulation of the blood is therefore more quickly arrested.

Dr. Burrows admits that he found the brains of sheep slaughtered by the butchers much less depleted than the brains of rabbits which have died from hæmorrhage. (p. 14.) In the former, as both carotid arteries and jugular veins are generally divided, and the animals also soon bleed to death, the cerebral vessels should, according to the view we have expressed above, appear to contain more blood. Dr. B., however, explains the difference, by asserting that "those sheep did not die from simple loss of blood; but partly from division of the pneumogastric nerves and cervical portions of the spinal cord." If the cervical portion of the spinal cord be divided, we presume the vertebral

arteries are also divided; and if the pneumogastric nerves be divided, we presume the carotids and jugulars are also cut across; and if all the arteries and veins leading to and from the interior of the cranium be severed, what possible influence can the division of the pneumogastrics and cervical portion of the spinal cord have in producing the effect assigned to it? Admitting that some of the vessels enumerated above were not divided, we deny that Dr. B. could adduce physiological data to show that simple division of the spinal cord and pneumogastric nerves would diminish the extent of the hæmorrhage, when such large vessels are cut across, as is done in slaughtering sheep. Let him analyze the known effects of such injuries upon the heart's action, and then reflect upon their probable influence under such circumstances.

In the disease termed anæmia, as the cases described by Hallè in the "Dictionaire de Médecine," by Dr. Combe and by Dr. P. M. Latham, and in individuals bled to death from repeated hæmorrhages, as detailed by Dr. Marshall Hall, when the blood of the body is not only deficient in quantity, but also in quality, or, in other words, contains not only absolutely, but relatively much less of the coloured part of the blood, and is more serous than natural, the brain, when examined after death, appears paler than usual. But does the cranium contain a smaller quantity of fluid? It would appear not; for, according to all the accounts given of those dissections, the vessels of the brain were always well filled, and have been sometimes described as congested, and never presented the shrunk and pale appearance observed by Dr. Kellie, in the brains of animals bled to death after a portion of the skull had been removed by a trephine.

That the blood-vessels within the cranium may appear better filled with blood, as in Dr. Burrows' experiments, in an animal suspended by the heels immediately after

death, than when suspended by the ears, though the quantity of blood in the vessels be the same, may also be explained. If the blood remain for some time fluid after death, the red particles being heavier than the liquor sanguinis will gravitate to the depending parts, and, consequently, the vessels within the cranium will contain a larger proportion of red particles, and will be more distinctly seen than when filled with a blood less abundant in red particles. It must further be remembered, that there is a great deal of vagueness in the use of such terms as "congestion of the blood-vessels within the cranium," and that we have no certain methods of ascertaining the normal quantity of blood in any of the organs of the body in individual cases. Every one must be satisfied, who has been much engaged in post mortem examinations, that the organs of the body contain naturally more blood in some individuals than in others, and this cannot always be explained by any particular kind of constitution or form of body. The difference in the quantity of blood found in the vessels of the encephalon, is sometimes very considerable in different persons, who had apparently died under nearly the same circumstances. There being a greater quantity of blood in certain vessels within the cranium is of course no proof that there is actually an increase in the whole; for suppose that the vessels on the surface were more injected with blood than usual, there may be less in some of the other vessels within the cranium,—a condition of the circulation which is quite in accordance with the doctrine, that the external surface of the encephalon is exempt from the influence of atmospheric pressure, as long as its osseous case remains entire. Besides, it ought to be remembered, that in attempting to estimate the quantity of fluids within the cranium, the venous sinuses ought invariably to be examined. When, therefore, there are so many sources of fallacy in judging of the quantity of fluids

within the cranium, we would, with all submission, suggest to Dr. Burrows a doubt whether the few experiments he performed entitle him to dogmatise so positively on this question; the more especially as they are at variance with the carefully conducted experiments and observations of other competent investigators.

We are very far from being satisfied of the truth of the remarks already quoted from Dr. Burrows, that "numerous observations show, that in the different kinds of death by asphyxia or apnœa, there is great congestion of the cerebral vessels, and that where it is absent, it may be accounted for on anatomical and physical principles;" and we feel convinced that this conclusion is not justified by the facts adduced by Dr. B. himself in support of it.

Certainly, if any circumstance could produce congestion of the vessels within the cranium, it would be that of death by hanging; for then the vessels, more especially the veins going to and coming from the brain, are compressed and then obstructed, except the vertebrals, which are protected by the peculiarity of their course through the foramina of the transverse processes of the cervical vertebræ. The vertebral arteries must continue for a time to force their blood upon the brain, while a comparatively small quantity only can escape by the veins; for the greater quantity of blood carried to the encephalon by the vertebrals returns by the internal jugulars, and not by the vertebral veins; and the anastomoses between the cranial and vertebral sinuses could carry off a small quantity only of the blood, transmitted along such large arteries as the vertebral. The vertebral veins are filled with blood from the occipital veins and veins of the spinal cord. Notwithstanding this, many accurate observers have declared that there is no congestion of the vessels within the cranium after death by hanging, however gorged the external parts of the head may be by blood and serum. We, ourselves, had an op-

portunity of examining the head of a stout muscular man of middle age, and previously in vigorous health, who committed suicide by hanging. The encephalon was not more vascular than usual, and the sinuses contained little blood, though the external parts of the head were gorged with blood and serum.

Dr. Burrows very ingeniously attempts to get rid of such troublesome facts, by supposing, that "in making such examinations, all the great vessels of the neck are usually cut across, and the thoracic organs removed from the body, before the head is examined; while the head is elevated during the operation of removing the scull-cap, and examining the brain, the fluid blood gravitates from the cranium," p. 27. He further supposes that the blood may gravitate downwards and diminish the quantity of blood within the cranium, when the head is placed in an elevated position, even when no incisions have been made into the body. The gravitating of the blood downwards from the vessels within the cranium, here assumed, is so obvious a *petitio principii*, a begging of the whole question at issue, that we need not dwell upon it. The other supposition, that the large vessels in the neck were cut previous to the opening of the cranium, will certainly not serve his purpose. The bodies of the criminals examined by Drs. Monro and Kellie, and in the case examined by ourselves, were intended for dissection, and were too valuable, at that period, to permit of such incisions being made in the neck, or the removal of the thoracic viscera.

Dr. Burrows is obliged to admit (p. 23) that "the appearances in the brains of those persons who die by hanging, would appear to support the opinion, that the cerebral vessels are not congested or overloaded in those cases where such a condition might be fairly expected;" but he immediately adds, "in opposition to such a conclusion, it would not be difficult to cite numerous well authenticated instances

of death by hanging, where the brain and its membranes have presented all the usual appearances of congestion, and even of apoplexy to a striking extent." He accordingly proceeds to cite such cases, and the first mentioned are examples of sanguineous apoplexy, which had occurred during the process of hanging, presenting all the usual appearances observed in the encephalon in that disease. That a blood-vessel should during hanging occasionally give way within the cranium, and that blood should escape when the vessels are preternaturally weak, is nothing more than what we should expect; for as it is more difficult to obstruct the passage of blood along the arteries than along the veins, and as it has been proved by experiment that in asphyxia, as the blood passing along the arteries becomes more venous, there is an increased pressure upon their inner surface,[1] if there be any tendency to sanguineous apoplexy, it is apt to occur at this particular time. These cases, therefore, stated by our author, have no bearing upon the question in dispute. The next and last case brought forward in proof of the statement cited above, is that of the Duke of Bourbon, the last of the Condés, alleged to have committed suicide by hanging. On examining the head, the vessels on the surface of the brain, especially on the anterior lobes, were gorged with dark *fluid* blood, and three ounces of serum were found in the lateral ventricles. Now, though the highly respectable and most intelligent medical men who examined the body gave it as their belief, that death was induced by the accumulation and stagnation of blood in the brain and lungs, we cannot, we think, be chargeable with presumption in expressing our dissent from this opinion as far as it relates to the brain. It is evident that there was atrophy of the brain in this case, as is proved by the presence of three ounces of serum in the

[1] Vide the paper on Asphyxia in this volume.

lateral ventricles, without flattening of the convolutions (for if these had been flattened, this fact would surely have been mentioned,) and if a part of the solids within the cranium had been removed, an increased quantity of fluid, either of blood or serum, would be present to maintain the plenitude of the cranium. We are, therefore, entitled to express doubt, that during the act of dying, if he did die by strangulation, any increase of fluid had taken place within the cranium. Besides, the reporters do not, in our opinion, state that there was an increased quantity of blood within the cranium; for the terms, accumulation and stagnation of blood, refer, we presume, only to the particular vessels mentioned, viz., those on the surface of the brain. Were not the appearances observed in the brain signs of chronic disease of that organ, which led to the committal of suicide, if really he died by his own hands and in the manner mentioned, for it at least was matter of doubt at the time? Considering that these are the only examples adduced by our author (for we throw aside mere opinions in the decision of a matter of fact,) we were not a little startled to find him hazarding the following statement:—"Enough has been said on this point of the pathology of the brain to prove, that in the majority of instances, when death takes place by strangulation, hanging, suffocation, drowning, and other means of causing apnœa, that a congestion of the cerebral vessels is found after death."

Our author has entered at some length into details relative to the cerebro-spinal fluid, and the influence which it exerts upon the cerebral circulation. (pp. 50-58.) "This fluid," he says, "is removable by pressure or absorption; at one time giving place to an increased quantity of blood in the cranium; at another making up for a deficiency of blood in the vessels in the head." That the cerebro-spinal fluid can, and does, pass between the spinal canal and cavity of the cranium, there can, we think, be no doubt;

and this admission does not invalidate the accuracy of the doctrine of the unvarying quantity of *fluids* within the cranium, as long as the solid parts remain the same. No doubt the propounders of this doctrine did not take this cerebro-spinal fluid into account in their illustrations of it, as at that time the existence of this fluid was almost entirely forgotten, for it was not until Magendie had again described its extent and position that modern anatomists fully understood its anatomy. We feel confident that our author has magnified the amount of influence which this fluid exercises upon the quantity of blood in the vessels within the cranium; at least there can be no question that he can adduce no proof of many of the statements he has advanced. It is well known that there is very little of this cerebro-spinal fluid, in fact in general little more than is sufficient to moisten the surface of the membranes, present in the interior of the cranium, in healthy persons, up to the middle period of life. Under these circumstances, the quantity of cerebro-spinal fluid that could be displaced from the interior of the cranium must be trifling. Suppose, on the other hand, a quantity of this fluid was to be forced up from the spinal canal into the interior of the cranium, from any morbid action going on in the former, a quantity of blood, equal to that of the fluid forced into the interior of the cranium, would be displaced from the vessels in that cavity. We are not, however, aware of any ascertained facts which prove, or even render it probable, that mere alterations in the distribution of the blood in the vessels of the cranium and spinal canal are of themselves sufficient to effect any very decided change in the relative quantities of the cerebro-spinal fluid that may at the time happen to be present in the interior of the cranium, and within the spinal canal, and the onus probandi rests upon those who assert that these do so.

With regard to the movements observed in the encepha-

lon, when the osseous case surrounding it is imperfectly formed, or when a portion of it has been removed—discussions upon which have been mixed up with the question we are now considering—we would make the following remarks. One of these movements is synchronous with the pulse, the other with expiration. The first depends upon an elevation of the entire brain by the fresh stream of blood driven into the large arteries at its base by each stroke of the left ventricle. The second depends upon the difficulty which the blood encounters in its free passage to the heart during expiration, especially during forcible and prolonged expiration, when the parts within the thorax are compressed, and there is consequent retardation and accumulation of blood (not reflux of blood, as Dr. B. asserts) in the veins leading from and within the interior of the cranium itself, when a portion of the skull is deficient. Can there be any such movements of the brain when the skull is entire? We think not; and for the following reasons:—If we were to remove a portion of the cranium, and carefully observe these motions, we would be convinced that they do not depend upon any recession of the brain from the inner wall of the cranium and its subsequent application—for the brain remains constantly in contact with the inner surface of the cranium—and they really consist of a slight protrusion of it through the opening in the parietes of the cranium, and its return to its former level. To us it appears obvious, then, that if there were no opening in the walls of the cranium, there would be no movement. These facts, however, sufficiently show the effects which long continued efforts, and the contractions of the heart, have upon the cerebral circulation. When the heart acts violently, the blood must be driven with greater force into the vessels at the base of the brain, and exert a greater pressure on the inner surface of the vessels, and consequently also upon the sub-

stance of the brain. And during a violent exertion, when the glottis is closed, and the parts within the chest violently compressed, the retardation of the flow of blood along the cerebral veins must increase the pressure upon the brain, and may assist in deranging the balance of the circulation, or causing rupture of the blood-vessels, though, as we have attempted to show, it may not, from certain physical conditions of the parts within the cranium, increase the quantity of blood there.

We have dwelt at such length upon the first part of this treatise, that we have space left for a hurried view only of the remainder. In the section (p. 80) in which our author treats of apoplectic cases, we find several statements from which we dissent; but as in many of these the points we have already discussed are involved, we pass them over. He attributes those cases of simple apoplexy, where no sufficient cause is found for the fatal result, to congestion, and, as his remarks imply, accumulation of blood in the vessels within the cranium; and he explains the absence of the signs of this increase of the quantity of blood in the vessels of the brain, by supposing that by the blood-letting, purging, posture, and other remedies employed, "the cerebral congestion, on which the apoplectic coma depended at the time of seizure, has been entirely dissipated." Of course, this statement can only be advanced as a supposition, and not as a fact, seeing that no proof is offered. We are aware of cases where the above explanation of the absence of increased quantity of blood in the cerebral vessels could not apply. We should like to have been favoured with Dr. B.'s explanation of the fatal result, notwithstanding that, before this took place, "the cerebral congestion on which the apoplectic coma depended has been entirely dissipated;" seeing that he learnedly quotes, a few pages after, this definition of the cause of a morbid action, with the view of clenching an argument on another point—

"Vera causa, præsens morbum facit; mutata mutat; sublata tollit."

We agree with our author in maintaining, as some previous writers have done, that we are not justified in attributing the coma in what is called serous apoplexy to the presence of the serum found within the cranium. At least this opinion cannot be disputed in those cases where the physical signs of undue pressure by increased effusion of serum upon the encephalon are wanting. These physical signs are easily recognised, and consist of a flattening and pressing together of the convolutions of the brain. Dr. B. attributes the coma in such circumstances to an accumulation of blood within the vessels of the brain, though he admits that this opinion is not based upon post mortem appearances. We cannot admit the validity of this supposition, and maintain that the condition of the brain which causes the symptoms in this form of apoplexy has not yet been fairly elucidated.

We were not a little surprised to find our author giving it as his opinion, "that apoplectic coma is rarely dependent upon the extravasation of blood, although the concomitant paralysis undoubtedly is." (p. 92.) This opinion is chiefly founded upon the circumstance, that the coma may disappear before the effused blood has been removed by absorption, and where we have the alleged cause existing, though the effect has disappeared. Now, this is not a fair statement of the facts of the case. The same cause acting on a substance will invariably produce the same effect, provided that the conditions of the substance acted upon remain the same; but if the conditions of the substance are changed, the effect will differ. Now, though the alleged cause, viz., the effused blood, may remain unchanged, does the condition of the substance acted upon, viz., the brain, undergo no change? Does not every organ in the living body accommodate itself, more or less, by virtue of its vita-

lity, to the circumstances under which it is placed? Suppose a person became comatose immediately after a blow upon the head, depressing a portion of the skull—and there are such cases on record—and that after a time the coma disappeared, though the depression of the skull remained unchanged, would it not be considered paradoxical to refer the coma to congestion of blood within the vessels of the brain,—the cause assigned by Dr. B. for the coma in sanguineous apoplexy,—and not to the depression of the skull?

We gladly leave this portion of Dr. Burrows' treatise, in which there is so much to blame, and turn to the second division of it, where there is much to commend. In the section on the treatment of apoplexy and hemiplegia, there are many judicious indications laid down; and the importance of attending to the state of the heart and lungs, especially the former, before deciding upon the line of treatment to be followed, is inculcated more strenuously, and on more conclusive data, than is to be found in any other author. We fully concur with him "in reprobating the indiscriminate use of the lancet in these cerebral affections." We have not been able to perceive the *rationale* of *all* the rules of treatment he recommends; but as the principal of these seem to be very judicious, we have no wish to discuss the value of the others. The extended, careful, and valuable researches made by our author on the connexion between affections of the brain and disease of the heart, are well deserving of the careful study of the practitioner; but the length to which our remarks have already extended, prevents us from entering upon this branch of his inquiry. In conclusion, we may state that the style of the book is excellent, and much superior to the generality of medical works. The language is generally good and appropriate, and there is no difficulty in understanding the author's meaning. Any one, however,

if hypercritically inclined, might point out a few slovenly expressions ; and in what piece of composition of the same extent may this not be done ? For example, the expression in the following quotation, which we have printed in italics, though admissible in conversation, ought not to be used in a professional treatise :—" She was conscious in the fit ; *upon coming to*, she found she had lost the use of the limbs of one side."

No. XXVI.

ANATOMICAL AND PHYSIOLOGICAL OBSERVATIONS ON SOME ZOOPHYTES.

(FROM THE ANNALS AND MAGAZINE OF NATURAL HISTORY FOR DECEMBER 1845.)[1]

In the following observations upon the structures and actions of some of the Zoophytes obtained from the shore of the bay of St. Andrews, I have confined myself to those points which are either new, or which appeared deserving of additional illustration. In using the terms *superior* and *inferior*, *upper* and *lower* in reference to the *relative* position of different parts of the polypidom, in the descriptive parts of this paper, the polypidom is supposed to be in the erect position, so that these terms correspond to *anterior* and *posterior* when the polypidom is placed horizontally. In using the term *anterior surface*, I mean the surface on which the apertures of the polype-cells are placed, so that this corresponds to the upper surface when the polypidom is laid horizontally for examination.

Cellularia reptans. This polype grows in considerable abundance close upon low-water mark, on the exposed

[1] A great part of this paper was read before the Literary and Philosophical Society of St. Andrews on the 30th Nov. 1844, and an abstract of it was printed in its Transactions.

surface of a stratum of clay-slate and conglomerate, interposed among strata of sandstone belonging to the carboniferous series. Growing along with it, but in much smaller quantities, are *Cellularia scruposa*, *Crisia chelata*, *C. eburnea*, *Pedicellina echinata*, *Vesicularia spinosa*, *Valkeria imbricata* and *Plumularia falcata*, none of which have I hitherto found adhering to the surrounding strata of sandstone.[1]

The polypidom of this polype possesses some structures which, as far as I am aware, have not yet been described. At the external and upper angle of the cell, and posterior to the two spines attached to this angle, (Pl. II., fig. 1 *a*, fig. 2 *c*, *a*, *b*), three of these structures are found. The uppermost of these is a hollow process (fig. 2 *b*),[2] the superior extremity of which is free, looks outwards and a little forwards, and has an aperture notched on the lower and upper edges, but more deeply in the former than the latter. From this aperture a hair-like prolongation (fig. 2 *d*), about the length of the cell, and slightly curved, projects. The interior of the process is filled with a fibrous contractile substance which moves this hair-like prolongation. Its movements occur at irregular, occasionally very short intervals, and it sweeps downwards over all the posterior surface of the polypidom within its reach. It then turns back upon its former track, ascending upwards until it reaches again the outer edge of that part of the polypidom lying above the process to which it is attached; it now descends in the opposite direction over the outer part of the polypidom, and places itself along the outer edge of that portion of the polypidom lying below it. From this

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[1] I have, since this was written, found the *Cellularia scruposa* adhering, in considerable quantities in one locality, to the sandstone.

[2] Part of this process is seen on looking at the anterior surface of the polypidom, as is represented in Plate II., fig. 3 *b*.

it re-ascends in the course just described. The extent of these movements is increased by the presence of the notches in the edges of the aperture through which the hair-like prolongation passes. These movements are perfectly independent of the polype, and continue for days after its death. The upper and outer edge of the polype-cell is prolonged into a process (fig. 1 *a*, and fig. 2 *c*) mucronated at its external and upper angle. This process is hollow, and is filled with a pale fibrous contractile substance, which I have frequently seen become elongated, and rise in the form of a short conical eminence above the upper edge of the process, and then after a while it contracted suddenly, and retired within the process. This process was in some cells metamorphosed into a strong spine, (fig. 1 *b*), and in such cases three spines were attached to the external angle of the cell instead of two, the normal number. It has an affinity with the tooth-like process of *Cellularia scruposa*, as both contain a similar contractile substance. Placed between the bases of the above two processes, and overlapping the latter, is a rounded small cavity with a distinct circular aperture (Pl. II., fig. 2 *a*). In some cells all these three appendices are wanting; in others only one of the two former is present. The polype protrudes itself through a small aperture directed outwards and upwards, placed at the upper end of the cell and towards its outer edge, (fig. 2 *a*), and immediately in front of the process bearing the hair-like prolongation (fig. 3 *b*). This aperture is crossed anteriorly by a pretty strong rim which forms the upper edge of the anterior surface of the cell, and posteriorly by the still stronger rim forming the upper edge of the posterior surface of the cell. Below this aperture there is a considerable portion of the anterior wall of the cell formed by a transparent membrane, and bounded by a thick edge, constituting the large oval opening in the anterior wall of the cell in dead or dried specimens. In the greater number of

cells this space is crossed by bars of calcareous matter, growing from its inner margin by one stem which generally divides dichotomously into four, and these increasing in length reach its outer margin (fig. 3 *a*). These bars are hollow, are lined internally by a fine membrane, and almost entirely disappear when the polypidom is immersed in dilute muriatic acid. Neither these bars nor the three appendices to the cells above described, present themselves until the body of the cell and its containing polype have been fully formed. The spines attached to the cell are almost always four in number,—two to each superior angle of the cell,—are hollow, and the external two are longer and stronger than the internal. The two former are of considerable thickness, and are generally as long, sometimes more than twice as long, as the cell.

The polype has from fourteen to sixteen ciliated tentacula, of a light orange-colour, rather more than three-fourths of the length of the cell. The animal when retracted within its cell is folded up as in *Flustra foliacea*. Fig. 5 is a representation of the polype when expanded, and fig. 4 represents its appearance as seen from the posterior surface when it withdraws and folds itself within the cell. In this polype the part marked *a* in the figures had more of the appearances of an appendix of the stomach (*b*), or of a separate organ, than in some of the other ascidian polypes.[1] Its inner surface is so thickly covered with reddish brown granules, or more properly speaking, minute cells, that it is quite opaque. Similar granules also adhered to the inner surface of the œsophagus (*d*) and stomach, and sometimes in greater number to the former than the latter. The inner surface of the pharynx (*f*), the œsophagus, the stomach, and a portion of the intestine (*c*) next the stomach, are

[1] From the contractility of these parts the shape is not uniform, and in some individuals we find the stomach less and the appendix larger than they are here represented.

covered with cilia. A mass of dark-coloured egesta, apparently principally composed of the cells and granules thrown off from the inner surface of the digestive tube, is frequently observed about or above the middle of the intestine, and this part of the intestinal tube presents a dilatation frequently considerably larger than is necessary to contain the inclosed mass. The polype in protruding itself first pushes out a short flexible tube attached to the inner margin of the aperture through which the tentacula pass. The muscles by which it withdraws itself within its cell are two in number,—one proceeding from the lower and outer part of the cell, and dividing into two bundles as it passes upwards, which are attached to the sides of the lower part of the pharynx; the other arising from the lower part of the cell, and attached to the lower end of the appendix of the stomach. (fig. 5 *a*.) The muscular bundles by which it protrudes itself cannot be distinctly traced from their proximity to the tentacula and intestine, but are seen passing downwards from the upper part of the cell along the sides of the tentacula to reach the gullet, and probably also the upper edge of the stomach. The flexible tube or operculum is retracted by two muscular bundles, one on each side, arising from the inner sides of and a little below the aperture of the polype-cell, and are inserted into the inner surface of the flexible tube. The young polype-cells, formed at the upper end of the branches, grow from the posterior surface of the polype-cells last formed a little below their upper margin. Their first appearance is that of a rough transverse line occupying the inner portion of that surface. Several specimens presented the bodies frequently termed opercula, but which we shall call *ovary-capsules*, placed as usual at the upper end of the polype-cells, and were here somewhat nearer their inner than their outer margins. The contents of these we shall describe in a subsequent part of this communication.

Cellularia scruposa. This polype is found, as I have already mentioned, in the same locality with *C. reptans*, and it is also thrown ashore from deep water, sometimes in considerable quantities and of more luxuriant growth, chiefly adhering to *Flustra foliacea* and *F. truncata.* A perpendicular hollow process springs from the upper and outer edge of the cell immediately above the already well-known tooth-like process, and adheres to the lower part of the outer edge of the cell immediately above. (fig. 6 *a*, and fig. 7 *b*.) The aperture of this process is pretty deeply notched before and behind, and its anterior is filled with a contractile fibrous substance which moves a curved hair-like prolongation (fig. 7 *b*) about the length of the cell, which sweeps at intervals over both the anterior and posterior surfaces of the polypidom within its reach. It rises up slowly over the anterior surface, makes a sudden jerk over the outer edge of the polypidom, and proceeds slowly downwards over the posterior surface as far as the notch in the aperture permits, and after remaining at rest for a longer or shorter time, it returns along the same course to the position from which it started. In this movement it performs a slight rotatory motion, so that its concavity is always directed towards the surface of the polypidom. This hair-like prolongation, in this as in *Cellularia reptans*, tapers gradually towards its free extremity, and is not rounded but flattened. In the *C. reptans* I never observed this hair-like prolongation cross the anterior surface of the polypidom, except when placed at the angle of the bifurcation of a branch. The use of these hair-like prolongations may probably be to keep the surface of the polypidom clear of substances which would otherwise adhere to it. Their motions are executed with more force than we should at first suspect. I have seen one of them in its course encounter the stalk of *Pedicellina echinata*, and press it aside. The tooth-like process (fig. 7 *c*) is hollow, has an aperture in its upper

edge, and in several specimens I have observed it filled with a fibrous contractile substance which expands and rises upwards through the aperture, and after remaining stationary for a time it re-enters the process. It rises only a short distance above the aperture, and when expanded presents the appearance of the upper and outer angle of the containing process with the curve turned in the opposite direction. When expanding it moves from without inwards, gradually rising above the edge of the aperture, and it re-enters the process by a sudden jerk in the opposite direction. These movements of expansion and contraction commonly occur after long intervals, and it is in general only by watching a portion of the polypidom for a considerable time under the microscope that they can be detected. More rarely these movements occur in rapid succession. I can form no conjecture regarding the function of this curious contractile substance. At the root of the process bearing the hair-like prolongation there is a small rounded cavity with an aperture in its posterior wall, exactly like that described in the corresponding position in *C. reptans*. (fig. 7 *a*.) Each cell has four small hollow spines attached to its upper edge, two adhering to each angle. These spines are very considerably smaller than those in *C. reptans*, and in old specimens are generally broken off. The position of the aperture in the cell through which the polype protrudes is similar to that in *C. reptans*, and is also provided with a short flexible tube, which acts as an operculum when the polype retires within its cell. Many specimens are provided with ovary-capsules placed as in *C. reptans*. The polype has generally twelve tentacula of a light orange-colour, and has in other respects a great resemblance to that in *C. reptans*, and is provided with the same muscular bundles for effecting its movements and closing the operculum.

Cellularia avicularis. I lately obtained a large and very

perfect specimen of this polype. The shape of the polype-cell, as Dr. Johnston remarks, is similar to that in *Flustra avicularis*. The bird-process is also exactly alike in both. It can, however, be readily distinguished from the latter by all the branches being composed of two rows of semi-alternate cells, and each cell having only two conical spines directed upwards or in the line of the long axis of the cells, and a little outwards and forwards, and attached to the angles of the superior margin of the cell. In a small number of cells an additional small spine, making three in all, projected from the outer angle in the same direction as the normal one. On the other hand, almost all the cells in *Flustra avicularis* have four spines, which differ in appearance from those of *Cellularia avicularis*. This specimen, when dried, assumed only a very faint ash-colour, very different from the much deeper ash-colour in all the dried specimens of *Flustra avicularis* I have seen.

These two polypes ought certainly to be classed as two different species of the same genus, and not under two different genera. A new genus should perhaps be instituted for their reception, as their general character, and more especially the possession of those remarkable appendices, the bird-head processes, separate them from *Acamarchis*, *Flustra* and *Cellularia*, the genera to which they are most allied.

Pedicellina echinata.—This polype is found in considerable quantities in front of the Castle of St. Andrew, and near low-water mark, adhering to *Cellularia reptans*, to *Sertulariæ*, and to the surface of stones. It is more hardy than most of the other ascidian polypes, and can be kept alive at home for a long time. The number of tentacula varies from fourteen to twenty. In some specimens the stalk is nearly smooth, in others several spinous-looking processes project from it, and in others both stalk and body are covered with a long, fine, and sparse down. In the

young animal the body is relatively longer and narrower. The body in the older animal is very decidedly compressed from before backwards and elongated transversely, and is considerably narrower and more bulging at the edge in which the intestine lies (fig. 8 *d*) than at the edge next the gullet. (fig. 8 *a*.) The upper part of the body is bounded by a slender rim to which the tentacula are attached. This rim slopes slightly from the narrow towards the broad end of the body. The tentacula at the extremity of the narrow end are shorter than the others, and all of them become considerably broader as they approach the rim. They are connected together at their lower third by a contractile membrane, partly composed of circular fibres. The body itself is not contractile. The inner surface of the edges of the tentacula and the inner surface of the rim are provided with strong cilia, and in the older animals the external surface of the tentacula is frequently covered with a layer of pretty large granules or cells. On examining the animal under the microscope when placed in water containing a quantity of carmine, the movements of the currents of water produced by the cilia can be more distinctly observed. The two rows of cilia attached to each tentaculum do not produce currents in opposite directions, but both strike downwards and towards the mesial line of the tentaculum to which they are attached, and cause a current down the centre of its internal surface, by which the particles of carmine are carried downwards to the rim. When all the currents carried down the tentacula arrive at the rim, they are rapidly conveyed along its upper edge by the action of the cilia with which this portion of the inner surface is so abundantly provided, towards the mouth. (fig. 8 *a*.) At this part all the currents converge, and thus produce an upward central current, by which the particles of carmine are carried outwards. None of the carmine, as far as I could observe, entered the œsophagus. The particles of

carmine sometimes collected in considerable masses around the mouth before they were floated outwards. As the termination of the intestine opens near to the mouth, and at a point within the influence of this outward central current, the egesta when voided are rapidly carried away. It would thus appear that when substances not fitted for the nourishment of the animal are conveyed towards the mouth, the walls of this aperture, from being endowed with a specific property of irritability, are thrown into contraction, and prevent its entrance. Such substances, on the other hand, as are capable of nourishing the animal do not act as excitants to this property of contractility, and they may be carried inwards. The possession of such a property is probably necessary for the existence of the animal. In this animal, as is well known, the whole digestive tube and the ciliary motions on its inner surface can be distinctly seen through the transparent body. The walls of the stomach (fig. 8 *b*) and the first portion of the intestine (duodenum?) (fig. 8 *c*) are very much thicker than the rest of the digestive tube, and were never observed to contract; and this last circumstance, viz., the non-contractility of these parts of the digestive tube, does not exist, as far as I am aware, in any other ascidian polype. A slight contractile movement was observed in a few cases at the upper part of the gullet. The last part of the intestine, (fig. 8 *d*,) which is not provided with cilia, contracts and expels the egesta which have previously accumulated there, frequently in considerable quantity. Brownish masses, apparently chiefly composed of the granules and cells which so abundantly line the inner surface of the stomach, are frequently seen in rapid rotatory motion in the stomach and duodenum.

The life of the body is of shorter duration than that of the stalk, and I have observed in several specimens the body fade and fall off, and a new one reproduced in its place. A few days before this takes place, the tentacula

are permanently bent inwards, and the membrane surrounding their lower part remains contracted, so as to completely, or nearly completely, cover the upper surface of the body, presenting in fact the appearance which the animal temporarily assumes when disturbed. The body then becomes more opaque, and at last falls off. After this the stalk retains its property of alternately contracting and relaxing its different surfaces at intervals, upon which its movements depend. After the lapse of a few days the top of the stalk enlarges, and a minute head presents itself in which the different parts of the body are developed. In the beginning of October I procured several specimens in which a large mass of cells (ova) was placed in the space between the gullet, intestine, and upper edge of the stomach, (fig. 8 *h*,) extending downwards to the entrance of the gullet into the stomach, and depressing the stomach, and forcing it considerably downwards. In two of these this mass of cells projected into the interior of the gullet near its lower part, and exceedingly minute ciliated ova were seen escaping from the upper part of the cellular mass, and several were also seen swimming in the interior of the gullet and stomach. Portions of this mass of cells were after a time extruded outwards, and were composed of the ciliated ova, and of very minute nucleated cells connected together by a structureless substance. Many of these ova formed a single cell, broader at one end than at the other, with a circle of cilia longer than the cell placed around the margin of the broad end, (fig. 9 *a*,) while others presented one, two, or more very minute cells attached to its lower or narrow extremity. (fig. 9 *b* and *c*.) The nucleated cells consisted of a cell-membrane with two or more nuclei, and appeared to be undeveloped ova. The ciliated ova swam actively about, sometimes bending all their cilia in the same direction, forming a curved bundle and striking in the same line for some time together, at other times

spreading their cilia and moving them in different directions. These ova are so minute as to require very high magnifying powers for their examination. It would thus appear that this polype, supposing all the individual animals whose stalks are attached to the same creeping stem to form one aggregate animal, extends and prolongs the life of the individuals composing it in two ways, viz., by renewal of the individual bodies after they have dropt off, and by offsets of new individuals from the creeping stem; and that it reproduces and extends the species, or forms new aggregate animals, by the formation of ciliated cells. I have never been able to detect any circulation of nutritious juices in the stalk, though examined under the most favourable circumstances.

Crisia chelata.—This polype when extruded affords a good view of the membrane connecting the outer surface of the pharynx and rectum together. (fig. 10 *a.*) It would be more correct to say, connecting the *supporting part* of the tentacula and rectum together, for the pharynx, as in the other ascidian polypes, lies loose, and can be seen contracting, within this supporting part. It protrudes itself through a small opening at the upper margin of the cell, and the large opening seen in the dead specimen on the anterior surface of the cell, is in the living specimen covered in by a membrane. The polype has from ten to twelve ciliated tentacula about half the length of the cell. The dilatation of the digestive tube (stomach) at the termination of the gullet and commencement of the intestine is smaller, and that part marked *a* in figs. 4 and 5 is relatively larger in this polype than in *Cellularia reptans* and *C. scruposa*, and has less the appearance of an appendix of the stomach.[1]

[1] As has already been stated, I have observed individual polypes both in *Cellularia reptans* and *scruposa*, but more especially the latter, where the difference between the size of the stomach and appendix was less marked than in figs. 4 and 5.

Its inner surface, however, is covered with a greater number of brownish granules than any other portion of the intestinal tube.

Campanularia dumosa. — I have procured some live specimens of this polype thrown ashore after a storm, attached to *Flustra foliacea.* The polypes and pith of the stalk are of a yellow colour. The polypes were sluggish, had twelve short tentacula not ciliated, and presented all the characters of the *Zoophyta hydroida.* Dr. Johnston writes me that he has also some time ago procured live specimens, so that he must be now aware that this polype cannot be a *Cornularia* as he once supposed, (British Zoophytes, p. 192, 1838,) and that the characters of the polypidom separate it from the genus *Campanularia.*

Alcyonidium parasiticum.—Abundance of this polype is occasionally thrown ashore chiefly adhering to *Sertularia argentea.* I have procured several specimens alive, and have satisfied myself that it consists of cells composed of animal and calcareous matter, and that the polype resembles the ascidian polypes in every respect. Mr. Hassall (Annals of Natural History, vol. vii. p. 370) first satisfactorily ascertained the true nature of this polype. On placing a portion of the polypidom under the microscope, and then bringing a quantity of dilute muriatic acid in contact with it, innumerable bubbles of gas are seen rising from all parts of its surface. On immersing another portion in aqua potassæ so as to destroy the animal matter, it lost its dirty brown colour, and the form and arrangement of the cells were then distinctly observed. Fig. 11 is a magnified view of a few of the cells in the portion of the polypidom thus treated. Each cell is provided with a flexible tube attached to its margin, which the polype extrudes before it emerges from the interior of the cell, and retracts when it re-enters, thus serving the purpose of an operculum. The first portion of this operculum extruded,

forms a small conical eminence with the apex truncated. When the polype withdraws itself within its cell, it frequently does not retract this portion of the operculum, so that the surface of the polypidom occasionally presents under the microscope a papillose appearance. The next stage in the protrusion of the polype is the elongation of this conical eminence by the eversion through it of a second portion, surmounted by pretty long setæ. The tentacula, by the upward motion of which the eversion of this flexible tube is effected, are now seen lying within it. The third stage in the protrusion of the polype is the passage of the tentacula and pharynx through the upper aperture of the flexible tube. The greater part of this tube appears to be composed of setæ connected together by a membrane. The polype has fifteen or sixteen tentacula. By breaking up a number of the cells I procured two of the polypes nearly entire, and the stomach and its appendix had nearly the same relative size as in *Crisia chelata*. Several bodies, each composed of reddish brown nucleated cells inclosed in a membrane (ova,) were seen among the broken-down polype-cells.

Flustra avicularis.—This polype is thrown ashore in great quantities after storms, chiefly adhering to the roots of *Flustra foliacea* and *F. truncata*. Each polype-cell has almost always four hollow spines, adhering to its upper margin, two to each angle. The two superior spines are pretty long and project upwards and outwards, and the two inferior, which are placed close to the two superior at their origin, are considerably shorter and less thick, and project generally inwards, forwards, and a little downwards. In a few cells I have seen five spines attached to the superior margin, three of these adhering to the outer angle. The bird-head processes attached to the outer edges of the branches of the polypidom are generally very considerably larger than those nearer their centres. Each bird-head

process may be described as being composed of a *body*, (fig. 12 *f*,) of a *hinge-process*, (fig. 12 *e*,) and of a *pedicle*, (fig. 12 *b*.) By the pedicle it is attached to the interior of a round hollow process projecting slightly from the anterior surface of the polypidom. (fig. 12 *a*.) The body of the bird-head process[1] is very convex along the lower edge, and it is elongated from below upwards, and somewhat flattened transversely. It is divided by an oblique ridge on its interior surface into two chambers, (fig. 12 *d*,) which communicate freely at the superior and middle parts at least. The hinge-process is articulated to the superior or concave surface of the body by a hinge-joint, along the line of the superior termination of the internal ridge which divides the body into two parts. The edges of the concave surface are thickened at this part, and present a slight depression on each, for receiving the two articular processes of the hinge-process. The body of the bird-head process is hollow, and its concave surface presents three apertures; the largest of these is the uppermost, and is separated from the middle by a bar stretched across between the articular cavities for receiving the hinge-process; and the smallest is placed at the lower end, and affords a passage to the posterior part of the pedicle into the interior of the body. The hinge-process is concave on its upper surface, and terminates below in a curved point. Its superior wall forming its concave surface is deficient in two-thirds of its length at the upper part or that next the articulation, and its inferior or convex wall is very thin over the same extent. It is hollow, and communicates with the body through the upper and middle apertures seen in its concave or upper surface. Its upper or articulating end is bounded by a thickened portion or bar passing between the edges of the

[1] In describing this moveable bird-head process, I have supposed the polypidom to be erect, and the concave surface of this process to be looking upwards in the direction of the long axis of the cells.

superior surface, and a similar bar passing between the edges of the inferior or convex surface. The articulating processes are placed upon the superior of these bars, at its junction with the edges of the superior surface. I have described, with what may appear very unnecessary minuteness, the skeleton of these bird-head processes, because it would be impossible to understand their movements without a previous knowledge of the different parts described. The lateral portions of the lower chamber of the body are occupied by two radiating muscles, presenting somewhat of the appearance of the temporal muscle in the human species, which converge at the articulating or upper edge of the hinge-process, and terminating in a denser, thicker, and narrower structure, which I shall call tendons, are attached to and move this process. (fig. 12 *c*.) One of these muscles, which is the stronger, terminates in a tendon which runs above the transverse bar which separates the upper from the middle aperture in the concave surface, and running down the centre of the hinge-process is inserted into the inner surface of its inferior or convex wall a little above its apex or free extremity. When this muscle contracts, the hinge-process is tilted up. The other muscular bundle, which is strongest at the upper and lower edges, terminates in a tendon which passes beneath the bar, and is inserted into the hinge-process close to and a little above the tendon of the other muscle. When this muscle contracts, the hinge-process, if elevated, is drawn down. The first described muscle is the *elevator*, the second is the *depressor* muscle of the hinge-process. The movements of the hinge-process are in general slight, but I have frequently observed it to be tilted up with considerable force, and closely applied over the superior surface of the anterior chamber, so that its concave, which was before its superior, became its inferior surface, and its convex became its supe-

rior surface. In this state it may remain for hours, and affords an excellent opportunity for observing the arrangement of the fibres of the two muscles, especially that of the elevator, as its lower fibres run more directly upwards, and its tendon is raised and separated from that of the depressor muscle. In dead specimens the hinge-process is not unfrequently found in the position into which it is brought by the action of the elevator muscle. These muscular fibres present no transverse striæ, can contract and relax with rapidity, and become shorter and thicker during their contraction. The movements of the body upon the polypidom are effected by the pedicle, and are as follows: —Suppose it to be attached to one of the edges of the polypidom, and the concave or upper surface to be looking upwards in the line of the long axis of the cells, it can turn slowly outwards over the edge of the polypidom until its concave surface looks directly outerward, and it then returns to its former place: it may also turn inwards until the concave surface looks across the cells. This movement being suspended, it exhibits at intervals a nodding motion, the concave and convex surfaces being alternately depressed towards the anterior surface of the polypidom. When the concave surface is carried downwards, the hinge-process is slightly separated from the body; but when the convex surface is depressed, it is again approximated. These last movements of the hinge-process are probably in a great measure mechanical, and occasioned by it rubbing over the surface of the polypidom during the downward motion of the concave surface. The pedicle consists of two parts: a posterior and dense portion which is attached to the internal surface of the inferior edge of the process of the polypidom to which it is fixed, and passes inwards through the inferior aperture in the concave surface of the body to be inserted into the lower part of the internal surface of the convex surface of the body; and an anterior portion, more

translucent and less dense, which is prolonged downwards into the process, and forwards to the middle aperture in the concave surface of the body and the attached end of the hinge-process. In the nodding movements when the convex surface is moved downwards, the posterior edge of the pedicle contracts and becomes bent so as to form an acute angle; and it relaxes while the concave surface of the body is moved downwards, resembling the contractile movements of the stalks in *Pedicellina echinata.* I have never had an opportunity of observing the changes in the pedicle during the other movements of the body under a high magnifying power, as this can only be done under certain conditions not easily to be obtained. The anterior portion of the pedicle has more of the appearance of a membranous than a contractile structure, and contains several small nucleated cells. A similar structure is found in the upper chamber of the body, and is prolonged through the upper aperture in the concave surface into the hinge-process. I have not been successful in observing contractile movements in this structure, if it really possesses this function, and I believe that it is more connected with the nutrition of these bird-head processes than with their movements. It would be very interesting to ascertain the functions of these complex appendices to the polypidom. Their movements are quite independent of the polypes, and continue for days after these are dead. The hollow processes of the polypidom, at least those next the outer edges, to which these bird-head processes are attached, spring from the upper surface of canals which communicate with the interior of the spines, the ovary-capsules, and also by lateral apertures with the interior of the cells next them. Can these organs assist in circulating water along these canals?[1]

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[1] This is a mere conjecture thrown out for future investigation.

The body of the polype is very small when compared with the length of the cell, so that when it enters the cell the gullet and intestine are not folded upon themselves as in *Cellularia reptans*, and so many other of the ascidian polypes, but are simply thrown into a curve. It has fifteen or sixteen ciliated tentacula considerably longer than the body: the cilia are short, thick, and numerous. In this polype, as in the *Crisia chelata* and *Alcyonidium parasiticum*, there is not so marked a division between the stomach and the part which has been termed the appendix, as in *Cellularia reptans* and *C. scruposa*. Brownish granules and minute cells are observed on the inner surface of the stomach, the gullet, and commencement of intestine. Ciliary movements are distinctly seen in this as in the other ascidian polypes examined, on the inner surface of the pharynx, gullet, stomach, and first portion of intestine. In some specimens the polypes were very active, darting back into their cells when disturbed, and immediately after again protruding themselves. When left undisturbed, they at short intervals partially withdrew into their cells, and immediately after again emerged and spread out their tentacula. The movements of the cilia attached to the tentacula appear to be in this, as in other ascidian polypes, under the control of the animal. They remain quiescent when the tentacula are withdrawn within the cell; and even when extruded their movements are occasionally for a time suspended. There can be no doubt that they can act also involuntarily, for they may be seen in full action upon detached portions of the tentacula. Very extensive contractile movements were very frequently observed in the pharynx, gullet, and stomach. The arrangement of the muscles, by the action of which the polype protrudes and withdraws itself within the cell, appears, as far as I could trace them, similar to those in *Cellularia reptans* and *scruposa*. The greater number of specimens were

provided with ovary-capsules, placed upon the thickened superior margin of the cells. In some specimens procured about the middle of October, these ovary-capsules were more or less filled with opaque bodies (ovaries) of a slightly yellowish colour. Each of these bodies was composed of small cells inclosed in a membrane. The external surface of this membrane was in many of them provided with cilia in motion, causing some of them to perform a rapid rotatory motion within the ovary-capsules. These ova in the first stage of their growth adhere to the upper end of the lining membrane of the capsule. This lining membrane stretches across the aperture in the capsule, and also sends a reflection across the cell immediately below the ovum so as to inclose it in a kind of sac, leaving, however, in the young ovum, a space between them. In the more advanced ova this membranous partition was much thickened, especially at the central part, forming a considerable projection in the direction of the aperture in the capsule, and contained a number of nucleated cells. When the ovum enlarges so as to fill the interior of the capsule, it pushes this membranous partition before it. This membrane was observed in a few instances where the ova were fully formed to contract and relax at intervals, and in this way it may assist in the escape of the ovum. On detaching some of the ovary-capsules with the view of examining their contents under a high power, one of the ova was seen partially extruded from the aperture in the capsule. It was divided by a deep fissure into two unequal parts, the largest of which was nearly entirely outside the capsule. (fig. 13 *a.*) The extremity of the largest portion (fig. 13 *c*) was distinctly prolonged, more translucent than the rest of the ovum, and presented along its free edge a row of hairs resembling cilia, which, however, remained quite motionless, while along the whole of the rest of the external surface of both portions, except upon the edges of the fissure, cilia were in such

vigorous action that it was impossible to distinguish them individually, and they produced the appearance of the rim of a wheel in very rapid rotation. After the lapse of an hour the fissure had extended through the whole body of the ovum, and the larger portion (fig. 13 *b*) being set free, swam about very actively in the water; but all this time the hairs attached to the prolonged anterior portion remained motionless. The smaller portion continued in the capsule, and performed very rapid rotatory movements. This was the only ovum I observed in the act of escaping from the anterior of the capsule, but I had an opportunity of watching three other bodies exactly similar to the larger portion of the ovum already described, when examining other portions of the same polypidom. One of these had become fixed, by the hairs attached to the anterior extremity, to a minute portion of sea-weed, and the cilia were in active motion. When examined ten hours after, the cilia were acting very languidly. I saw another while swimming about become entangled by its cilia to the setæ projecting from the body of a small annelide. During the movements of the annelide, the hairs on the prolonged anterior extremity came in contact with some small fragments of sea-weed, and the annelide after some struggles detached itself from the ovum, which continued to adhere to the sea-weed. In all of these I never observed the least movement of the hairs attached to the anterior extremity. I was not able to ascertain that the smaller portion of the ovum left in the capsule underwent any change, as I presume it does, before it escaped from its interior. Several bodies, having one portion of their surface ragged and devoid of cilia, and in every other respect resembling the smaller portion of the ovum, and also other bodies exactly similar to the entire ovum, were observed swimming about; but as in all these cases the portions of the polypidom had been injured immediately before, and some of the ovary-capsules broken, it

was presumed that these had been mechanically displaced from the capsules. The larger portions of the ova were, like the entire ova, composed of minute cells, and did not, as far as I could discover, possess any internal cavity.

In several specimens of *Cellularia reptans* and *C. scruposa*, and one specimen of *C. avicularis* procured at the same time, the ovary-capsules were filled with ova; in the two former of a deep orange colour, composed of nucleated cells, having the same number and arrangement of membranes, and provided with cilia, as in *Flustra avicularis*. Some of these ova were in rapid rotatory motion; others, as in *Flustra avicularis*, were motionless, though the cilia were acting, being kept quiescent by the more close apposition of the inclosing membrane. I did not succeed in observing the escape of any of these ova from their capsules.[1] In many of the polype-cells of all of the above-mentioned polypes, dark red bodies composed of nucleated cells inclosed in a membrane were present. These nucleated cells are generally considerably larger than those entering into the formation of the ova in the ovary-capsules. The greater number of polype-cells contained one only of these bodies, and it was connected to the inner surface of the cell by a membrane having a number of detached nucleated cells of a light colour adhering to it. These bodies occupied different positions between the bottom and aperture of the cells, but in none were distinct ciliary motions observed. These bodies are also probably ova, and it is possible that more extended observations may enable us to detect cilia on their surface at a more advanced stage of development, though none in the present case were seen even on those lying at

[1] [During the last twelve months I have been anxious to procure some of these bodies, which I suppose may be ova, for the purpose of examining their constitution with more powerful glasses. I do not feel satisfied that the description here given, of the cells being nucleated, is correct.]

the aperture of the polype-cells. I have satisfied myself that in the polypes mentioned above, the inner surfaces of the polype-cells, of the appendices of those processes described in the *Cellularia reptans* and *scruposa*, of the bird-head processes, of the spines, and of the canals running along the lateral surfaces of the polypidom in *Flustra avicularis*, are all lined by a fine membrane. This membrane in old specimens, and when the polypes are dead, often presents numerous and pretty large cells, generally of a pale colour, at other times having a slightly yellowish or brownish tinge, adhering to its free surfaces. In one specimen these cells had accumulated in such quantities within some of the spines in *Flustra avicularis*, as to produce considerable bulgings and excrescences. The growth and nutrition of the hard parts of the polypidom must be chiefly due to this membrane.

EXPLANATION OF PLATE II.

Fig. 1. Magnified view of the posterior portion of the upper end of a branch on the polypidom in *Cellularia reptans*. It is slightly elevated on the left side, so that the polype-cells of that side are better seen than on the other.

Fig. 2. Three appendices to the cells in *Cellularia reptans*.

Fig. 3. Magnified view of four polype-cells of *Cellularia reptans* seen on the anterior surface.

Fig. 4. Magnified view of polype in *Cellularia reptans* when folded up in its cell.

Fig. 5. Magnified view of this polype when expanded.

Fig. 6. Magnified view of the anterior surface of the upper part of one of the branches of the polypidom in *Cellularia scruposa*. The polype-cells are in this drawing also more distinctly seen on one side than on the other.

Fig. 7. Magnified view of three appendices to the polype-cell in *Cellularia scruposa; b, b bis*, views of the process bearing the hair-like prolongation in two different positions.

Fig. 8. Greatly magnified view of head and upper part of stalk in *Pedicellina echinata*.

Fig. 9. Greatly magnified view of the ciliated ova of *Pedicellina echinata*.

Fig. 10. Magnified view of polype in *Crisia chelata*.

Fig. 11. Magnified view of polype-cells in *Alcyonidium parasiticum*.

Fig. 12. Magnified view of bird-head process in *Flustra avicularis*.

Fig. 13. Magnified view of ova in *Flustra avicularis*.

No. XXVII.

ON THE DEVELOPMENT OF THE OVA OF THE NUDIBRANCHIATE MOLLUSCA.

(FROM THE ANNALS AND MAGAZINE OF NATURAL HISTORY FOR JUNE 1846.)

THE following observations upon the development of the Nudibranchiate Mollusca were made on the ova of the *Doris bilamellata*, *Doris tuberculata*, *Goniodoris Barvicensis*, *Polycera quadrilineata*, *Dendronotus arborescens*, *Doto coronata*,[1] and a species of *Eolis* having numerous flattened papillæ, depressed and imbricated, arranged in fifteen or sixteen distinct rows, which I have not been able to refer to any of the species of that genus, the descriptions of which have yet come under my notice.

In the middle of last September I procured several specimens of *Polycera quadrilineata* found together near low-water mark, in each of which there was placed immediately beneath the external integuments of the upper surface a large white mass, slightly lobulated, lying behind and on each side of the heart, and prolonged forward as far as the external orifice of the reproductive organs. Two pairs of the animals were kept in separate vessels and

[1] In naming these animals I have followed Messrs. Alder and Hancock in their "Synopsis of British Nudibranchiate Mollusca," contained in their beautiful monograph published by the Ray Society in 1845. These gentlemen consider the *Goniodoris Barvicensis* to be a variety of *Goniodoris nodosa*.

daily supplied with fresh sea-water. Eight days after this one of the pairs was seen *in coitu*, lying side by side, the head of the one looking towards the tail of the other, and having the right edges of their bodies in close apposition. When examined twenty-four hours later neither had spawned, but two hours after this one had spawned, and the other was in the act of spawning. The spawn was seen to issue very slowly from the dilated vagina, and the animal very gradually shifted its position to permit the spawn to assume a ribbon-form, and cause it to adhere by one of its borders to the object upon which it was deposited. The process of spawning did not seem to occupy any very great length of time. The other pair was seen *in coitu* nine days after I had taken them home, and when examined thirteen hours after this neither had spawned, but two hours later one had spawned, and the other was spawning. The animals by spawning became considerably reduced in size. They were kept alive for three weeks, and they deposited small portions of spawn between ten and fourteen days after the first spawning. It does not, however, appear to be absolutely necessary for the production of fertile ova in all, if in any of the individuals of the Nudibranchiate Mollusca, that a coitus should have so shortly preceded spawning as was observed in the *Polycera*, for an *Eolis* which was kept strictly confined in a vessel by itself, deposited, on the tenth and again on the thirty-second day of its isolation, abundance of fertile ova. During the high spring-tides at the end of last February, I found near low-water mark several large assemblages of *Goniodoris Barvicensis* and *Doris bilamellata* among the rocks, collected for the purpose of breeding.[1] In one of these groups there must have been at least between sixty and seventy individuals of

[1] From the unusual mildness of this spring, the breeding-season may have commenced earlier than usual.

the *G. Barvicensis*, and abundance of their spawn adhered to the surface of the rocks, and in one place a portion about six inches square was almost completely covered by it. Many of the *Doris bilamellata* had also spawned, and were collected in smaller, more numerous and scattered groups, the greater number of which were farther from low-water mark and in more exposed situations than those of the *G. Barvicensis*. These assemblages do not break up for some time, but continue to occupy nearly the same position, and the animals composing them spawn more than once. I found some individuals still lingering among the rocks, and recent spawn deposited, as late as the end of April. Several pairs of the *Doris tuberculata* were also observed, and I procured four specimens of *Dendronotus arborescens*, and two specimens of the *Eolis* mentioned above. The individuals of the two last genera mentioned were not found in pairs, and these, along with several specimens of *Goniodoris Barvicensis* and *Doris bilamellata*, were taken home and kept until they had spawned. About the same time I procured several specimens of *Doto coronata* from the deep sea adhering to *Plumularia falcata* and *Thuiaria thuia*, which were also kept alive, and began to spawn about the middle of March. Near the end of March I found a considerable quantity of the spawn of the *Doris tuberculata* adhering to the under surface of the ledges of rock near low-water mark.

The spawn of the *Doris bilamellata*, *D. tuberculata*, *Doto coronata* and *Polycera quadrilineata* is of a ribbon-shape and of a white colour, adhering by one of its edges to the substance upon which it is deposited; while in the *Goniodoris Barvicensis*, *Dendronotus arborescens*, and in the *Eolis*, it assumes more or less the shape of a rounded cord. The ribbon-shaped spawn of the *Polycera quadrilineata* formed a small segment of a circle, and measured between half an inch and an inch in length, and about three-

twelfths of an inch in height; that of the *Doris bilamellata* a large segment of a small circle, measuring in some cases eight-twelfths of an inch in height; while that of the *Doto coronata*, wound in a spiral manner round the branch of the zoophyte to which it adhered, and its free edge, instead of being straight as in the other ribbon-shaped spawn mentioned, presented under the microscope numerous and regular convexities. The spawn of the *Doris bilamellata* consists of two laminæ of a structureless substance, between the layers of which, except for a small space near the edges, very numerous small ova are placed. These ova are firmly fixed in their position by a substance of the same nature as the external laminæ, which unites them together, and fills up the interstices among the ova. The ribbon thus formed is very elastic and tough, but when the ova are about to escape it becomes soft and gelatinous, the laminæ separate at the edges, and at a later period may give way at different parts. This description applies to the spawn of the *D. tuberculata* and *Doto coronata*, and probably also to the *Polycera quadrilineata*, but my notes do not enable me to speak positively of this last. The spawn of the *Goniodoris Barvicensis* is either white or of a faint pink colour, has a disposition to assume the semicircular form, and some portions were from one to two inches in length, and from a quarter to nearly half an inch in circumference, and though rounded, it shows a slight tendency to the ribbon form. Its structure is essentially the same as that of the *D. bilamellata*, with this difference, that the external lamina approaches the cylindrical form, with the ova arranged in the centre. The spawn of the *Dendronotus arborescens* was in the form of a small, long, and waving cord, destitute of the strength and elasticity of the ribbon-shaped spawn, of a faint pink colour, attached to a branch of a *Plumularia*, along the side of which it formed a series of festoons. The circumference of this cord was formed of

a transparent membranous-looking substance having no distinct structure, and the ova which occupied the interior were easily displaced and forced outwards. The spawn of the *Eolis* was deposited upon the inner surface of the vessel in which it was kept and upon a stone, and consisted of a small cord of a pale pink colour, about one-twelfth of an inch in diameter, arranged in a close spiral form, the turns of which lay almost in apposition, and were surrounded and connected together, and to the surface of the object upon which they were placed, by a transparent structureless substance. This cord when arranged in this manner made up a larger cord from three-twelfths to four-twelfths of an inch in diameter, and several inches in length, which formed several coils, some of which crossed or were superimposed upon others. The structure of this cord was the same as that of the *Dendronotus arborescens.*

When the ova are examined under the microscope soon after the extrusion of the spawn, each is seen to consist of a thin transparent membranous case, (Pl. III. fig. 1 *a*,) with a round, smooth, and opaque body in its centre. (fig. 1 *b*.) This membranous case, (chorion,) which I shall designate the *case-membrane*, is of a circular or oval form, is larger than what is sufficient to contain the opaque body within it, and its walls appear to be composed of at least two distinct laminæ.[1] The opaque body within is of a round form, and is chiefly composed of minute cells, (nuclei,) intermixed with a structureless substance which I suppose to be semi-fluid, and the whole is enclosed in an external transparent membrane (vitelline membrane.)[2] I shall restrict the term *ovum* to this opaque body inclosed in the *case-membrane*.[3] The ova

[1] I observed these laminæ separated from each other in some parts by a distinct interval in several ova of the spawn of the *Eolis*.

[2] Fig. 2 is a diagram showing the external or vitelline membrane imperfectly filled with its contents.

[3] No doubt the *case-membrane* is an accessory part of the *ovum*, and it is for convenience sake that I use the latter term in this restricted sense.

of the *Doris bilamellata* vary in size from about 1-250th to 1-280th of an inch in diameter, those of the *Eolis* were nearly of the same size, while those of the *Doris tuberculata* were considerably larger, and those of *Doto coronata* smaller than this. The minute cells (nuclei) composing the greater part of the vitelline mass are of a round or oval shape, (fig. 3,) vary in size in the *Doris bilamellata* from 1-6000th to 1-9000th—the greater number being from the 1-7000th to 1-8000th—of an inch in diameter, and no nucleoli were observed in their interior. A very great number of these ova were examined when subjected to very different degrees of pressure, and their structure appeared to be uniform, presenting no differences at different parts, and entirely composed of the materials we have described. I endeavoured to discover a clear cell in the centre of the vitelline mass, similar to that described by Kölliker and Dr. Bagge as existing in the ova of different species of *Ascaris*, and which plays so important a part in the cleaving of the yolk; but if such a cell exists, it escaped my notice from the opacity of the yolk. No evidence of the presence of this cell was obtained in any of the subsequent changes through which the ovum passed. In the spawn of the *Polycera quadrilineata*, *Doto coronata*, *Doris bilamellata*, *D. tuberculata*, and *Dendronotus arborescens*, one ovum is generally contained in each *case-membrane*; but in some portions of the spawn of the *Doris bilamellata*, two and even three ova were found in the same *case-membrane*. The greater number of the *case-membranes* in the spawn of the *Eolis* contained each two, three, four, and even five ova. The size of the case-membranes varies according to the number of ova which it contains. In the spawn of the *Eolis* the shape of the *case-membranes* is easily altered by external pressure, so that instead of being circular or oval, they were frequently multangular.

One and sometimes two small transparent cells were

seen in some of the ova examined soon after being spawned, adhering feebly to the outer surface of the external membrane (fig. 2 *a*) at the line of the first division of the ovum. These transparent cells were in general very easily detached by pressure, and were sometimes seen lying loose within the *case-membrane.*

I shall first describe the changes observed in the ova of the *Doris bilamellata* in the course of their development. A few hours after the extrusion of the spawn, a transverse groove presents itself on the surface of the ovum, (fig. 4,) and this gradually deepens and divides it into two equal parts, each part being of a circular form, and completely inclosed in its own external membrane. (fig. 5.) Each of these again undergoes a bipartite division, (fig. 6,) and the four equal parts of which the ovum now consists proceed in their turn to divide in the same manner. (fig. 7.) In a portion of spawn examined between eleven and twelve hours after its extrusion, all the ova, with the exception of a very few which presented the primary division into two, had divided into four parts; and eight hours after this, or about nineteen hours from the time of spawning, most of them had divided into eight, some still consisted of four, and a few of six parts. Examined after twenty-seven hours from the time of spawning the ova had the appearance represented in fig. 8, and after fifty hours the appearance represented in fig. 9.[1] At the end of about seventy-five hours the cells of the ovum were still more subdivided, as is shown in fig. 10. Between this period and the end of the fifth day, the division of the cells of the ovum appeared to have reached its utmost limit, and the ovum gradually changed its shape, becoming some-

[1] It is necessary to employ a fresh portion of spawn in each observation, as the development of the ova in those portions of the spawn used in such examinations under the microscope is very seriously disturbed.

what elongated, and broader at one end and narrower at the other, as in fig. 11. Up to this time these cells, though adhering by those parts of their external surfaces in contact, could sometimes be detached from each other by the application of pressure—more readily, however, at the earlier periods of their division; but after this they began to coalesce, though some might be separated from the rest still later. Each of the small cells into which the ovum had divided measured from the 1-1500th to 1-2000th of an inch in diameter, and consisted of a cell-membrane, with several of the minute cells of which the ovum was originally chiefly composed placed like nuclei in its interior. (fig. 12.) The ovum at the time of its extrusion may perhaps be considered to be a single large cell with a great number of minute cells or nuclei in its interior. During the subdivision of this large cell into a number of smaller cells, I could detect no changes, after repeated examinations, in the form of the minute cells or nuclei, and none in their arrangement which were not apparently produced by the bending inwards of the external or vitelline membrane to effect the subdivision. I do not mean to assert that these central nuclei were not efficient agents in producing these changes; I only wish to state that I was unable to detect any alteration in the form or in the arrangement of these nuclei preceding these subdivisions. At the end of the sixth day no additional change had taken place in the external form of the ovum, but the cells into which it had divided were continuing to coalesce, and minute cilia were observed on the upper surface of the broad extremity. On the eighth day it had assumed the form represented in fig. 13; its circumference had become somewhat translucent, especially at the lower and middle parts, where the external layer of cells had separated themselves from the others, and coalesced to form the commencement of the shell. (fig. 13 *a.*) The incipient shell contained many of the minute cells or nuclei,

and bands of them passed between different parts of its inner surface and the dark mass in the interior. The cilia on the broad extremity had become larger and more active in their movements, and traces were observed of the division of this end into the *ciliated discs* (fig. 13 *c*) and the *foot*. (fig. 13 *d*.)[1] The cells into which the ovum had divided had now almost disappeared, while the minute cells or nuclei of which the ovum originally chiefly consists seemed to be as numerous as ever, and were diffused, except where the shell was forming, through a glutinous-looking structureless substance. It is now entitled to the term of *embryo*. Instead of describing in their order of succession the different changes through which the embryo passes until it leaves the case-membrane and swims at large, I shall first describe its structure at that period, as this will save a good deal of repetition, and also render the description more easily followed. Some of the embryos left their *case-membrane* about the end of the fourteenth day after spawning, but the whole had not escaped until three or four days later. The *case-membrane* previous to the escape of the embryo becomes gradually thinner, and at last either entirely disappears or is reduced to shreds. This change in the *case-membrane* may probably be in some measure caused by the incessant strokes of the long cilia of the *ciliated discs* upon its inner surface during the active revolutions of the embryo round its interior. The embryo at the time of its liberation is provided with a shell, (figs. 25 and 20 *a*,) considerably longer in its antero-posterior than in its transverse diameter, from which it can protrude the upper part of its body and retract it at pleasure. Fig. 20 is a representation of the embryo when protruded from its shell, and fig. 21 when entirely drawn into its interior. The embryo with its shell is at

[1] These parts are indicated by the same letters of reference in a more advanced stage of their development in fig. 17.

this period considerably larger than the ovum at the time of its extrusion. Some of them measured about 1-145th of an inch in length, and 1-200 in the antero-posterior diameter; others 1-170th in the former and 1-250th in the latter direction. The parts which can be protruded from the shell are two large and prominent ciliated discs, (fig. 20 *c*,) and a projecting process[1] attached to the upper part of the anterior surface of the body. (fig. 20 *d*.) The lower surface of this projecting process or *foot* is covered by a hard plate, (fig. 20 *g*,) which closely adheres to it and moves along with it. The *ciliated discs* are higher in front than behind, and are separated in front by a deep notch and by a shallower one behind. They are very contractile, and present very different appearances at different times; and their superior surface is provided with a thickened margin, to the upper and outer edge of which a row of long and strong cilia is attached, by whose movements they can swim rapidly in various directions through the water. When these discs are elevated and in the vertical position, as represented in figs. 20 and 18, the parts connecting their margins to the body of the embryo are translucent, and they may now be contracted from before backwards and a number of the central cilia collected into a tuft; or if the embryo be about to retire into the shell, they are pressed together, the translucent texture connecting their thickened margins to the body contracts and pulls them downwards, and the *foot* with its hard plate is raised, as is represented in fig. 21. This plate now acts as an imperfect operculum. When the embryo, on the other hand, is about to swim, the *ciliated discs* are thrown apart and flattened, as is represented in figs. 16 and 19; and in this position each disc approaches the circular form,

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[1] To an exactly similar structure in the embryo of the *Asplysia* Van Benedeu (Annales des Sciences Naturelles, tom. xv. p. 123, 1841) has given the name of *foot*, and we have here retained the appellation.

is hollow on the upper surface, and their thickened margins are prolonged inwards along the edges of the anterior notch, at the bottom of which they are continuous. In the bottom of this anterior notch, immediately in front of the point where the thickened margins of the discs become continuous, and at the base of the upper surface of the *foot*, the mouth, (fig. 16 *y*,) which is formed by a simple rounded aperture, is placed. The long cilia attached to the outer edge of the upper surface of the thickened margin of the discs are, when at rest, first bent inwards at an acute angle as far as the inner edge of the thickened margin, and then project upwards and outwards, but become more straight when in a state of action. The upper surface of the *foot* and the sides of the mouth are provided with cilia considerably smaller than the locomotive ones attached to the margin of the disc, and still smaller cilia are placed upon the hollow upper surface of the discs and other parts of the embryo outside the shell. Two very obvious and transparent cells, (figs. 16 and 17 *x*,) possessing much more refractive power than the other parts of the embryo, are placed in the base of the foot, at the sides of the gullet and immediately below the mouth. Each of these is apparently inclosed in a larger cell; at least each of them is surrounded by a well-defined ring, which, however, is more opaque than the cell which it encircles. (figs. 20 and 21.)[1] From the mouth the gullet leads downward and forwards to the stomach, (fig. 20 *h*,) and from the back part of the stomach the intestine (fig. 20 *i*) commences. The intestine bends to the right, proceeding upwards on this side, and termi-

[1] Van Beneden supposes that these transparent cells which he observed in the embryo of the *Asplysia* may be the rudiments of the nervous system. This opinion may be true, but at present it must be considered only in the light of a supposition. These cells, if I mistake not, may be occasionally seen vibrating slightly within the larger cells inclosing them. It has been suggested to me that these may be the rudiments of the auditory organs.

nates a little below and behind the right transparent cell in the root of the *foot*, and it is there surrounded by a portion of an irregular mass composed of a few cells (fig. 20 *o*) occupying that position. The whole of the inner surface of the gullet, stomach and intestine, is covered with cilia, and in some cases, masses, chiefly composed of what appeared to be minute cells thrown off from the inner surface of the digestive tube, were revolving rapidly in the stomach. Two masses adhered to the lateral surfaces of the stomach and lower part of the gullet; (fig. 20 *m* and *n*;) one of these, by much the larger, (*m*,) was placed on the left side, and projected considerably in front of the stomach; the other adhered to its right side, (*n*,) and was placed immediately in front of the upper part of the intestine. Each of these two bodies was composed of a single cell only, having minute cells or nuclei similar to those originally composing the ovum, scattered over its inner surface with considerable intervals between each. In several cases some minute bodies were seen moving backwards and forwards in the interior of the largest of these two cells.[1] On the right side there is an irregularly-shaped aperture, (fig. 16 *t*,) immediately behind the termination of the intestine, and between the upper part of the body and the aperture of the shell, through which the water passes into the interior, and cilia were seen in active motion in this situation. A band passes from the upper part of the mass of cells placed at the termination of the intestine, round the neck of the embryo, close to the margin of the aperture of the shell, and forms the outer boundary of this opening, by which the water passes into the interior of the shell.[2] As the largest

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[1] The nuclei adhering to the inner surface of the larger of these two cells appeared bigger than the nuclei of which the ovum was originally chiefly composed, and also than those in the other parts of the embryo.

[2] This band is more distinctly seen in the embryo of the *Dendronotus arborescens.*

of the cells, placed at the termination of the intestine, was seen to contract at irregular intervals, I imagined that it might be a rudimentary heart, and the band to be a vessel leading from it, but I obtained no satisfactory evidence of the accuracy of this supposition. A pyramidal-shaped mass projects from the upper and back part of the body, (fig. 20 *s*,) from the apex of which a thin membrane descends and passes round the body. (fig. 20 *v*.) A strong band of contractile fibres is attached to the lower part of the left side of the shell, and passing up on the same side, divides into two portions, which terminate upon the back part of the neck and gullet. A very minute band passes from the same part of the shell to the lower part of the stomach. These muscular bundles, though distinctly seen, especially the strong band passing upwards, in the embryos of all the Nudibranchiate Mollusca examined, are remarkably distinct in that of the *Dendronotus arborescens*. (figs. 22 and 23 *p p*.)[1] It is by the contraction of these muscular bundles that the animal retreats into its shell, causing the descent of the posterior portion of the *ciliated discs*, and the parts to which the muscular fibres are attached. I could not make out the position and course of the muscular bundles by which the embryo protrudes the *ciliated discs* and *foot* from the shell. The upper and anterior part of the body, a little below the base of the *foot*, is attached to the anterior margin of the shell, so that it undergoes little change of position during these movements of retraction and extrusion. The whole structures of the embryo are much more transparent than at an earlier period of its development, no minute cells or nuclei now adhere to the inner surface of the shell, and their number in the other parts is much diminished. The other parts of the embryo appear to be formed of a soft texture without any definite structure, having minute cells

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[1] This muscular bundle is indistinctly indicated in fig. 19.

or nuclei scattered at short intervals through it. Some of the embryos escape from their *case-membranes* at an earlier stage than this, and their structures are consequently more opaque.

We now return to the earlier stages of the development. At the end of the ninth day the embryo had the appearance represented in fig. 15. The *ciliated discs* (*c*) and *foot* (*d*) were now distinctly seen, the central mass had separated itself to a greater distance from the shell, (*a*,) except at the upper part, and the portion within the shell had arranged itself into four imperfectly-defined lobes, which are readily recognised to be earlier conditions of the structures described in the embryo when it is about to leave the *case-membrane.* The ciliated discs were, however, still small, but the embryos had now a greater or less degree of motion; some performing a rocking motion, and others more advanced were rotating slowly round the interior of the *case-membrane.* Besides the large cilia on the margins of the *ciliated discs*, smaller cilia were observed on different parts of the upper end of the embryo. The mouth was distinctly seen, the hard plate on the lower surface of the *foot* had begun to form, and the transparent cells in the base of the foot were seen on subjecting the embryo to pressure. A layer of the minute cells or nuclei covered the inner surface of the shell, giving it a considerable degree of opacity. From the ninth to the eleventh day the *ciliated discs* had become more developed, more separated from each other, and much more moveable. The largest of the four lobes of the body had arranged itself into a stomach and intestine, and occasional contractile movements were seen in these; and the transparent cells in the base of the *foot* were now very obvious. On the twelfth day the embryo had assumed the appearances represented in figs. 16, 17, 18, and 19, and all the parts described in it at the time of its leaving the spawn were now distinctly seen. Fig. 16 is an

anterior view, showing that the two large cells at the side of the stomach are at this period connected by a ridge running across the front of the gullet, and which afterwards nearly disappears. The larger or left cell does not at this period project so much in front of the stomach, and the right cell is larger and lies near the anterior edge of the stomach. Fig. 17 is a view of the right side, showing the whole course of the intestine; fig. 18 is a view of the left side, in which the commencement of the intestine, (*i*,) curving itself to the right side, is the only part of that tube seen; and fig. 19 is a view of the posterior surface, showing the tortuous course of the intestine, (*i*.) The cilia on the inner surface of the stomach and intestine are now also visible. The course of the gullet is not yet distinctly seen, and all the structures are much more opaque. The loose membrane surrounding the body at a later period (fig. 20 *v*) adheres to the inner surface of the shell, and like the other soft textures and the retractor muscle, contains numerous small cells or nuclei. At this period the embryo was never seen to draw the *ciliated discs* within the shell.

The ova of the other Nudibranchiate Mollusca examined passed through the same stages of development as those of the *Doris bilamellata*, and the embryos presented, with some slight modifications in size and position, the structures we have described. The embryo of the *Doris tuberculata*, at the time it leaves the *case-membrane*, is larger than the others, measuring about 1-100th of an inch in length and 1-130 in the antero-posterior direction; that of the *Dendronotus arborescens* measured 1-165 in length, and 1-250 in the antero-posterior direction; and that of the *Doto coronata* about 1-200 in the former and 1-260 in the latter direction. The shell of the embryo of the *Doris tuberculata* is relatively shorter in length or in the vertical direction than in the other embryos examined, and the parts which protrude beyond the shell are not only posi-

tively but relatively larger. The *ciliated discs* are especially large, the apex of the *foot* is narrower and more pointed, and the transparent cells in the base of the *foot* are relatively smaller. The gullet and stomach are short, and the two large cells at their lateral surfaces are placed near each other at a later period of its development than in the *D. bilamellata.* The cells at the termination of the intestine are more numerous and transparent, and occupy a considerable space of the upper part of the right side of the shell, so that the body of the embryo lies more to the left than to the right side of the mesial line of the shell.

The cilia were not observed on the upper part of the ovum of the *Goniodoris Barvicensis* and *Polycera quadrilineata* until the seventh day, or one day later than in that of the *D. bilamellata,* and the embryos of the *Polycera* did not begin to leave the spawn until the eighteenth day. The transparent cells in the base of the *foot* are, from the less opacity of the body, seen at an earlier stage in the embryos of the *Polycera,* (fig. 14 *x*,) and in a great number of these also at this stage, an opaque irregular patch, composed at least partly of aggregated cells, lay on the surface of the lower end. (fig. 14 *b*.) The development of all the ova of the same spawn does not proceed *pari passu,* but much greater irregularities were observed in the ova of the *Polycera quadrilineata* than in the others; for in many of these the bipartite division did not even proceed regularly, nor were the cells into which it divided of the same size. That these irregularities were not entirely dependent upon the artificial conditions under which the spawn was placed, is rendered probable by the circumstance that they were seen in portions of spawn, in which, to judge from other portions of the same spawn examined at a later period, all these irregularities disappeared at a more advanced stage of their development. The external form of the embryo of the *Dendronotus arborescens* presented a much greater departure

from that of the *Doris bilamellata* than any of the others. Fig. 22 is a representation of the left side, and fig. 23 of the right side of the embryo of the *D. arborescens* as it was leaving the *case-membrane*. The shell (fig. 24) was more elongated in the vertical direction, the embryo occupied a smaller portion of the shell, and the parts which project beyond it were relatively considerably smaller. All the textures were transparent, and the retractor muscles were very distinctly seen. The membrane surrounding the body (*v*) was attached to the shell around the origin of the retractor muscles. When it retired within the shell, the *ciliated discs* and *foot* were drawn down to a considerable distance from the orifice of the shell. When examined at an earlier period of its development, the whole embryo was decidedly shorter and much less transparent. I have had no opportunity of examining the embryos of the *D. arborescens*, except when developed under artificial circumstances, but the embryos possessing the appearances described, seemed healthy and active.

To what extent the artificial circumstances under which the ova of these animals were kept influenced the period of time occupied in their development, we are not prepared to form an opinion. That the changes of structure described are those that occur in ova of the *Doris bilamellata* and *D. tuberculata* when left in the situations where they are usually deposited, was proved by the examination of portions of the spawn removed at different periods after deposition upon the rocks. The development of the ova of the *Doris bilamellata* proceeded more favourably than that of the others; but sometimes a considerable number even of these had their development arrested, and otherwise rendered monstrous, though supplied daily with water fresh from the ocean. I have as yet failed, though I have made the attempt in various ways, to keep the embryos alive after they leave the spawn, sufficiently long to trace the further stages

of their development. Sars[1] and Messrs. Alder and Hancock[2] have already announced that the young of the Nudibranchiate Mollusca undergo metamorphosis, that they swim about for a time inclosed in a nautiloid shell, and that at this period they differ entirely in their external form and in their organism from their parents.

I may here mention that three of the *Dendronotus arborescens*, which I kept alive at home for nearly a month, often emitted very audible sounds, which were heard distinctly at the distance of twelve feet. Dr. Grant, who first noticed these sounds, supposes that they are produced by the action of the jaws. They exactly resembled the noise produced by a stroke upon the surface of the earthenware vessel in which they were kept, so that I at first imagined that it might be caused in this way, though it would be difficult to conceive how these animals could strike blows so forcible as to occasion so loud a sound. I, however, heard these sounds, when it appeared to me that the animals were removed from the surface of the vessel and resting upon the branches of some zoophytes. Messrs. Alder and Hancock mention that they have frequently kept these animals for several days together without detecting the emission of any sound. It is possible that the animal emits this sound only during the breeding season.

EXPLANATION OF PLATE III.

Fig. 1. Ovum of *Doris bilamellata*; *a*, case-membrane (chorion;) *b*, yolk or vitelline mass.

Fig. 2. Part of the vitelline mass evacuated, showing the vitelline membrane; *a*, small clear cell, sometimes seen attached to the outer surface of the vitelline membrane.

Fig. 3. Small cells (nuclei) forming the greater part of the vitelline mass.

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[1] Wiegmann's "Archives" for 1841. I have not seen Sars's paper, and quote this reference to it from Alder and Hancock.

[2] Report of the British Association for the Advancement of Science, volume for 1844, p. 27.

Figs. 4, 5. First or bipartite division of the yolk.

Figs. 6, 7, 8, 9, 10, 11. Subsequent divisions of the yolk.

Fig. 12. Greatly enlarged view of one of the numerous cells into which the yolk ultimately divides.

Fig. 13. Embryo on the eighth day after extrusion of the ova.

Fig. 14. Embryo of *Polycera quadrilineata* at the same period of its development.

Fig. 15. Embryo of the *Doris bilamellata* at the ninth day.

Fig. 16. Anterior view of the embryo of the *Doris bilamellata* at the twelfth day.

Fig. 17. View of right side of the same.

Fig. 18. View of left side of the same.

Fig. 19. Posterior view of the same.

Fig. 20. View of right side of the embryo when it is ready to leave the *case-membrane.*

Fig. 21. View of the same when it has retired within its shell.

Fig. 22. View of left side of the embryo of *Dendronotus arborescens* when about to leave its *case-membrane.*

Fig. 23. View of right side of the same.

Fig. 24. Anterior view of shell of *Dendronotus arborescens.*

Fig. 25. Lateral view of the shell of *Doris bilamellata.*

No. XXVIII.

OBSERVATIONS ON THE DEVELOPMENT OF THE MEDUSÆ.[1]

(EXTRACTED FROM THE ANNALS AND MAGAZINE OF NATURAL HISTORY FOR JANUARY 1848.)

THE following observations were made upon three colonies of the larvæ of a *Medusa.* One of these was procured on the 15th of September 1845, and the other two on the 11th of July 1846, adhering to the lower surface of stones in pools near low-water mark. The stones were of a size which readily permitted them to be conveyed home, where I have kept them up to the present time.[2] The mode I have followed in keeping these animals alive is this. The stones to which they adhere are placed in vessels of considerable size, supplied daily with water fresh from the ocean, and the animals fed once or twice weekly with small morsels of mussels, which they readily swallow. The first of the three colonies consisted of between thirty and forty individuals, and the largest was between two and three lines in length; the individuals composing the other two colonies were more numerous and of somewhat larger size.

[1] These observations were laid before the Literary and Philosophical Society of St. Andrews at the Meetings of the 4th of May 1846 and the 5th of April 1847, and abstracts of them were printed in the "Transactions" of the Society, and reprinted in Nos. 118 and 131 of the first series of this Journal.

[2] [I have kept some of these larvæ alive up to September 1848, when they were still in an apparently healthy condition.]

After I had completed my examination of the structure of these animals, I discovered that they had been described by Sars, first under the generic name of *Scyphistoma*, and afterwards as the larva of the *Medusa*.[1]

Many of the larvæ increased much in size several months after I took them home, and the body of one that I measured was one-third of an inch in length and one-fifth of an inch in diameter; another was 4-12ths of an inch in length and 9-12ths of an inch in circumference. As every part of their body is contractile, they can assume a great variety of forms. The more common of these are represented in Pl. IV. figs. 1, 2, 3, 4, and 5. Though almost all of them are throughout of a greyish white colour, a few presented spots or patches of a purple colour, which were sometimes observed to disappear and reappear in the same individual. The tentacula are generally from twenty-two to twenty-seven in number, and when fully expanded are three or four times the length of the body. In one that I measured the body was 4-24ths of an inch, and the tentacula 12-24ths of an inch in length; in another the body was 2-24ths, and the tentacula 8-24ths of an inch in length. The mouth is very dilatable and varies much in shape, but is most commonly quadrangular. When fully expanded, it forms a round aperture occupying nearly the whole of the disc, (fig. 5;) at other times its margins or lips are elongated and approximated so as to form a considerable quadrangular projection. (fig. 2 *b*.) Its more common condition perhaps is that represented in fig. 3 *a*.

The four round, equidistant, and slight depressions placed between the mouth and margin of the disc are represented in fig. 2 *a*.

The body and tentacula of the larva are composed of two distinct layers, an internal and external. The internal

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[1] Annales des Sciences Naturelles, tom. xvi., p. 321. 1841.

layer chiefly consists of nuclei and nucleated cells (Pl. V. fig. 19) of various sizes, some of them containing a large number of nuclei; while the external is chiefly composed of a structureless substance with numerous minute nuclei disseminated through it. Numerous nearly elliptical and oval capsules, (filiferous capsules,) having a long thread or filament coiled up in the interior of each, are fixed upon the outer surface of the external layer, and in much smaller number upon the inner surface of the internal layer, where it lines the internal cavity or stomach. These capsules are most abundant upon the external surface of the tentacula. Fig. 20 is a highly enlarged view of a small portion of one of the tentacula, showing the filiferous capsules attached to its outer surface. These filiferous capsules vary much in size, but the largest are generally of a uniform size, nearly of an elliptical form, and about $\frac{1}{2000}$th of an inch in their largest diameter. (Pl. IV. fig. 8 and Pl. V. 23.) Several of these, detached in examining portions of the larva under the microscope, had burst open at the smaller end, and the spiral thread projected through the opening and was uncoiled. (fig. 9.) In the entire capsule a rounded and narrow column passes from the smaller end, beyond which it slightly projects, in the direction of its longest diameter, nearly to its other extremity; and this column, to which the spiral thread is attached, protrudes from the interior of the capsule when it bursts. I have never observed these filaments projecting from the capsules when adhering to the surface of the body, unless when subjected to pressure, but it is difficult to use the more powerful object-glasses necessary for distinguishing these, without compressing more or less the part under examination.

The internal is considerably thicker and more opaque than the external layer, is of a slightly yellowish colour when it accumulates at any point in greater abundance than usual, and is folded inwards to form the four equi-

distant projections seen on the surface of the stomach when the mouth is dilated, (fig. 5 *a*,) and when the body of the animal is slit open and then spread out. (fig. 6 *c*.) By making a transverse section of the body, the relative thickness of the internal and external layers, and the manner in which the internal is folded to form the four pouches or short canals that project from the internal surface, are very distinctly seen. (fig. 7.) These four short canals (fig. 7 *a*) terminate at their upper end in another canal, encircling the mouth and placed between it and the margin of the disc. (fig. 6 *b*.) Into this circular canal the hollow tentacula also open. The inner surface of the circular canal and the tentacula is lined by the internal layer. The four depressions (fig. 2 *a*) placed between the mouth and margin of the disc correspond to the termination of the four vertical in the circular canal. Across the bottom of these depressions, which at first sight look like apertures, a membrane is stretched sufficiently thin to permit readily of the transudation of fluids.

After reading Steenstrup's observations on the structure of these animals,[1] where he describes four canals—one in each angle of the extensible membrane surrounding the mouth and forming the lips—passing from the circular canal already mentioned, and also another circular canal placed in the free margin of the lips, I repeated my examinations; and though I used glasses of very different magnifying powers, and made numerous trials, I could not satisfy myself of the existence of these canals. No doubt four equidistant white lines presenting the appearance of canals are seen, in certain conditions of the extensible lips, running in the positions indicated by Steenstrup; but in some of the numerous forms which the lips assume these

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[1] On the Alternations of Generations, &c., translated for the Ray Society, pp. 22, 23.

lines entirely disappear, and when present they seem to be formed by narrow ridges on the external surface, resulting from the quadrangular shape assumed by the lips. The free margin of the lips frequently presented indications of the presence of a canal, but I could never satisfy myself of its actual existence. In making such investigations, it must be kept in mind, that the internal is readily separated by pressure from the external layer, otherwise we may be led into error. In the almost daily examinations I have made of these animals during the last two years, I never observed the slightest traces of the hollow quadrangular body described by Steenstrup as growing from the lower surface of the cavity or stomach in the body of the animal, sometimes projecting as high as the mouth, and placed in the middle of the stomach, like the clapper in a bell.

The inner surface of the lips and of the stomach, and the external surface of the tentacula and body, are covered with very fine cilia, so that currents of water, unless when the mouth is shut, are constantly passing in and out from the mouth and along the tentacula. The cilia upon the external surface of the body require the use of the higher object-glasses for their detection, and for a long time they escaped my notice.

The colony of larvæ first obtained began to produce buds and stolons about the middle of January 1846, and the other two colonies at the end of July of the same year. With intervals of comparative repose they have gone on reproducing abundantly ever since; so that, notwithstanding they are constantly suffering loss by death and other causes, the number of individuals in each colony has greatly increased. Whenever buds and stolons are formed, they commence by a thickening of the internal layer at those parts, causing a bulging outwards of the external layer. A single bud, (fig. 10 *a*,) occasionally two buds, grow from

the upper surface of the stolon, and these become developed into larvæ in the manner described by Sars. The buds form upon all parts of the external surface, but most frequently near the lower part of the body. On many of the larger larvæ several buds were seen growing at the same time. (fig. 11 *a.*) As a bud enlarges it becomes elongated and attenuated at its free extremity, and bends itself downwards to reach the surface of the stone to which the elongated extremity adheres: after this the attached end is gradually separated from the body of the parent. When thus detached, a small opening presents itself at its upper end, its interior gradually becomes hollowed out and cilia grow upon it: and tentacula commence to sprout around the mouth, exactly in the same manner as in the buds formed on the upper surface of the stolons. The outer surface of the buds is also covered with very fine cilia. Several of the buds were found lying loose at the bottom of the vessels in which the stones are kept, probably detached by accident, and these after a time fixed themselves to the surface of the vessels, and passed through their development into larvæ in the same manner as those that adhered for a longer time to the bodies of their parents. One of these detached buds fixed itself at two separate points, and two mouths, each furnished with its own tentacula, were formed at opposite ends of its upper surface. When a bud was developed on a stolon, the connecting part between the bud and the parent was more frequently absorbed, or at least disappeared, at other times the bond of connection remained; so that occasionally two, three, or more larvæ of different or of nearly equal size might be seen growing closely united together at the base, as if one had split itself longitudinally into two or more separate individuals. This chiefly took place when the larvæ were so thickly clustered together that they had not room to spread sufficiently. When the buds were developed into

young larvæ, these generally moved outwards from their parents to a small distance, leaving room for those that were to succeed them. This locomotion is generally slow—one larva that I watched moved 6-24ths of an inch in fourteen days—and is effected by a sliding motion of the attached end over the substance to which it adheres. In this motion the attached end bulges outwards in the direction it is about to take, (fig. 12 *a*,) and the whole of this end gradually follows, carrying of course the whole of the upper part of the body along with it. More rarely they move more rapidly by pushing outwards a narrow prolongation similar to a long stolon, (fig. 4 *a*,) which becomes fixed at its further extremity, and the attached end of the body becoming loosened, the whole is carried onwards by the contraction of the prolonged part. The older larvæ are almost or entirely stationary.

The larvæ, when detached from the surface to which they are adherent, can again fix themselves. I have frequently performed this experiment by placing those detached in separate vessels, and almost always successfully, when care was taken to disturb them as little as possible for three or four days, or longer. A considerable number of larvæ are adhering to the surface of the vessels in which the stones are kept.[1]

I made several experiments upon the reparative powers of the larvæ. In several the upper half of the body was cut off, and after three or four days its lower or cut end had closed in, and by the sixth day it had attached itself to the surface of the vessel, and shortly assumed all the appearances of an entire larva, sending out stolons and form-

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[1] According to Sars, " si on détache violemment ces polypes, il n'y a qu'un petit nombre qui peut se fixer de nouveau, et alors ils n'adhèrent pas si fortement qu'à l'ordinaire ; la plupart restent libres au fond du verre."—*Opus cit.*, p. 339.

ing buds. Fig. 12 is a representation of the upper half of a larva eight days after it had been cut off. New tentacula, and a new mouth also, after several days, presented themselves on the upper or cut end of the lower half. Several were divided longitudinally through their entire length, and when means were not taken to keep the cut edges apart they soon adhered again, and no traces of their division remained. In one divided longitudinally the two portions were kept apart, and in each the cut edges approximated and adhered, and two separate animals were thus produced from one.

The larvæ are voracious, and readily seize and swallow univalve or bivalve molluscans, or a crustacean, as large or even larger than their own bodies before they are stretched out, and after retaining them in the stomach, generally for about twenty-four hours in summer and nearly twice as long in winter, they reject them through the mouth. They also not unfrequently swallow one of their neighbours, and its sojourn in the stomach for some time terminates in its digestion and destruction. When they seize a univalve molluscan too large to be swallowed, they retain it firmly embraced in their tentacula, and insert their elongated mouth into the interior of the shell; and in like manner they keep dead articulate animals, or molluscans without shells, too large to be swallowed, in their tentacula for more than a day, and probably extract nourishment from them by acting on their textures by their extensible lips.

The larvæ of the first colony, obtained in September 1845, did not split transversely into young Medusæ in the spring of 1846, as I expected them to do, but continued to produce stolons and buds abundantly. A great number of them had then attained a large size, and many of them presented on their outer surface transverse rugæ, and four pretty deep equidistant vertical grooves, as represented in fig. 13, but none of them presented the appearances now

about to be described, indicative of their splitting transversely into young Medusæ. In the beginning of February of the present year, the upper part of the body of some of the larvæ of the first colony became cylindrical, considerably elongated and much diminished in diameter, with thickly-set rings forming at the top. From the circumference of the rings first formed eight equidistant lobes or rays began to grow, the rings increased in size, and became of a reddish brown colour, the tentacula gradually wasted away, and in the course of eight days the young Medusæ were beginning to detach themselves in the manner described by Sars. While this was going on at the upper part of the body, the process of elongation and the formation of new rings was proceeding downwards, as represented in Pl. V. fig. 14, so that thirty or forty rings, each of which was about to become a young Medusa, could be counted on the body of one larva at the same time, and the body in some cases measured three-fourths of an inch in length. At this period the upper part of the body was of the form of an inverted pyramid, and had a distinctly reddish brown colour. As the grooves separating the rings increased in depth, it was observed that the body of the young Medusa above was at last attached only to the upper margin of the lips of the one below. Fig. 15 is a greatly enlarged representation of one of these young Medusæ immediately after it had separated itself from the body of a larva. A small proportion, probably not above one-sixth or one-seventh of the larvæ, underwent this process of splitting into young Medusæ, and in no case that I observed did it extend through the whole length of the body of the larva; for a portion, often very small, at its attached end did not become ringed (fig. 14 *a*,) threw out new tentacula before the young Medusæ last formed were detached, and it continued to live as a larva. Some of the larvæ of the other two colonies obtained in July of the pre-

ceding year began to yield young Medusæ about the middle of March, and exactly in the same manner as in the first colony. A fortnight, or more, generally elapsed, after the commencement of the separation of the young Medusæ in a larva, before the process was finished.

The general appearance and habits of the young Medusæ immediately after they have detached themselves from the larvæ have been described already by Sars, but there are various parts of its structure which stand in need of additional elucidation. External to the quadrangular mouth occupying the centre of the lower surface of the body of the young Medusa (fig. 15) are four bifid hollow processes, placed at equal distances from each other, and adhering by the end of their undivided portion to the inner surface of the inferior wall of the stomach. (fig. 15 *a*.) The inferior wall of the stomach, which forms also the inferior surface of the body, is so thin that at first sight these processes appear to be attached to the external surface. Fig. 16 is a greatly enlarged view of one of these bifid processes. Each of these processes forms two hollow floating tubes, communicating with the stomach or internal cavity by a common orifice, (fig. 16 *a*,) and having the edges of their external surfaces covered with numerous filiferous capsules. (fig. 16 *b*.) The stomach is large and extends nearly to the margin of the body or disc. Outside the position of the four bifid processes, and on the lower surface of the inferior wall of the body, there is a circular band, slightly elevated, more granular and opaque than the portion of the body placed within it, having prolongations passing off from its outer edge to the intervals between the eight bifid lobes or rays that spring from the margin of the body, and others along the centre of the lower surface of these bifid lobes, as far as the ocellus placed at the point of bifurcation of each lobe. (fig. 15.) When the animal contracts the marginal lobes in swimming, this circle becomes narrower, more

distinctly defined, and approaches nearer to the mouth. In certain states of the animal the prolongations from the outer edge of this circle to the intervals between the eight bifid rays are longer than represented in fig. 15. When the animal is examined in certain positions and with glasses of weak power, this circle, and the sixteen prolongations extending outwards from it to the intervals between the rays, and along the lower surface of the rays themselves, assume pretty nearly the appearances represented by Steenstrup as vessels; and as I have been unable to satisfy myself of the presence of any vessels there, I am inclined to believe that he has been misled in this way. I have occasionally observed the appearance of a thread-like nervous circle around the mouth, sending a filament along each of the rays towards the ocelli, on approaching which it bifurcated; but not having been able to make these out at other times, under circumstances that appeared favourable for their detection, I am not prepared to affirm that a nervous system is present.

At the point of bifurcation of each of the marginal lobes or rays there is placed, as Sars has described, a little eminence, hypothetically designated by Steenstrup an ocellus. (figs. 15 *c* and 17 *a*.) This ocellus forms a mammillary process, consisting of three distinct structures. (fig. 17 *a*.) The apex is chiefly formed of a considerable number of very minute crystals, and a small part of its base is more opaque and more granular than its larger middle portion. From a greatly enlarged view of the crystals occupying the apex of the ocellus, given in fig. 18, it will be observed that the upper are shorter and thicker than the lower; in fact, while a few of the former are almost as thick as they are long, some of the latter are almost needle-shaped. On fixing the polarizing apparatus to the microscope, it was observed that these crystals depolarized the light. I gave some of the young Medusæ to Principal Sir David Brewster

for examination, and he returned me the following report: "The small raised portions of the Medusæ named ocelli, consist each of six or more similar parts, each part having the property of depolarizing polarized light. When all the other portions of the animal are absolutely black, the ocelli shine with considerable brightness. Upon turning the Medusæ round in a plane perpendicular to the axis of vision, the individual parts of the ocelli disappear and reappear, according to the angle which their neutral axis (if they have double refraction) or their planes of separation (if they are merely polarizing laminæ) form with the plane of primitive polarization. If these raised portions named ocelli are really organs of vision, the probability is that their axis of vision is perpendicular to the general surface of the Medusa."

The inner half of the lower surface of the bifid portion of each of the marginal lobes (fig. 17) is thinned off to a sharp edge, bounded externally by a continuation of the ridge running along the middle of the inferior surface already described, so that the bifid portion resembles in form the cutting portion of a pair of strong scissors.

A number of larger and smaller filiferous capsules, similar to those observed in the larvæ, adhere to the outer surface of the young Medusæ; and fine cilia are present on the inner surface of the lips and stomach, and on the outer surface of the four bifid processes floating in the stomach.

Though the normal number of the marginal lobes or rays is eight, yet occasionally they were as few as four and as many as twelve. In a few cases one or more of these lobes were trifid, with an ocellus placed in the cleft of each division.

I was not able to preserve the young Medusæ alive more than twenty days. During that time the lobes or rays had apparently become shorter from the expansion of the body, and in a few, small papillæ were forming in the clefts between the lobes.

A comparison between the observations of Sars and Steenstrup upon the larvæ of the Medusa living in the ocean, and those made upon them while living in the artificial condition described, elicits some facts of considerable interest. According to Sars and Steenstrup, the colonies of these animals living in the ocean split up entirely into young Medusæ each spring, and completely disappear, and new ones are founded in September from the ova of the adult Medusæ; but while living in the artificial state, as was also some years ago remarked by Sir John Dalyell,[1] a certain number only of the individuals of the colony undergo this process, and that not throughout their entire length; for even a portion of each of those that form young Medusæ by transverse divisions of their substance, continues to live as a larva. The first colony I obtained was seventeen months in my possession before any of the individuals composing it underwent their development into young Medusæ. That the larvæ, even when living in the ocean, are not always formed in autumn and undergo their development into young Medusæ in spring, is evident from the fact, that two of the colonies in my possession were obtained from the ocean in July. Whether these larvæ had been generated the preceding autumn, and continued to live as such up to the time they were obtained from the ocean, or had been generated at some period subsequent to this, it is impossible to determine.

ACCOUNT OF A NEW ACTINIA.

Though the *Actinia* I am about to describe has in many respects a close resemblance to the *Actinia chrysanthellum* of Mr. Peach, described and figured in Dr. Johnston's late edition of his work on 'British Zoophytes,' vol. i. p. 220,

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[1] Jamieson's Philosophical Journal for 1836.

it yet differs from it sufficiently, at least as far as I can make out, to justify me in regarding it as a distinct species. If this should be confirmed, I would propose to name it *Actinia cylindrica.*

Body elongated, cylindrical, free; tentacula uniserial, submarginal; mouth elongated upwards, forming a conical tube with small processes attached to its margin.

This animal was found in St. Andrew's Bay, by Mrs. Macdonald and myself about two years ago, immediately after it had been thrown ashore during a storm, and it was kept alive for three days. Fig. 21 (Plate V.) is a representation of the form of the animal of the natural size.

The body is cylindrical and marked by longitudinal lines. The inferior fourth of the body is translucent, more contractile than the upper part, and sometimes assumes nearly a conical form with the apex downwards. The upper three-fourths of the body are opaque and of a faint pink colour. The tentacula are twelve in number, ranged in a single row, smooth on the surface, of a light pink colour, and having their internal or oral surface crossed by four zigzag white lines. (fig. 22.) They are elongated transversely or flattened from within outwards, and taper towards their free extremity. They were never seen more elongated than what is represented in fig. 21, but as the animal appeared to be languid, it is quite possible they are capable of greater elongation. When contracted to the utmost they formed little conical eminences, projecting outwards and upwards, and were seen to be attached immediately below the outer margin of the disc. Twelve bands of a faint reddish brown colour and adhering along their edges, radiate inwards from the circumference of the disc, converge at its centre, and prolong themselves upwards to form the mouth, or rather the lips. The margin of the lips is surrounded by twelve small processes, six of

which are very minute; these processes are of a triangular form and of an orange colour, except at the edges, which are translucent. This prolonged mouth did not always occupy the centre of the disc, but could be directed towards any part of the margin.

The external sac sent strong partitions inwards, the position of which was marked by the longitudinal lines on its outer surface, and in the interstices of these partitions the ovaries were placed. This animal in many respects closely resembles the *Iluanthos Scoticus* of Professor E. Forbes,[1] and the chief difference between them is found in the structure of the mouth.

EXPLANATION OF PLATE IV.

Figs. 1, 2, 3, 4, and 5. Representations of the more common forms assumed by the larvæ.

Fig. 6. A larva slit open and stretched out to show the four vertical canals, and the manner in which they terminate in the circular canal: *a*, extensible lips; *b*, circular canal; *c*, four vertical canals; *d*, tentacula considerably shortened by their contraction.

Fig. 7. Transverse section of the body of a larva to show the manner in which the four vertical canals are formed: *a*, vertical canals.

Figs. 8. and 23. (Plate V.) Filiferous capsules entire.

Fig. 9. Filiferous capsule burst and the spiral filament uncoiled.

Fig. 10. Larva throwing out stolons, from one of which a bud is springing.

Fig. 11. Larva having several buds growing from its surface.

Fig. 12. Upper half of a larva eight days after it had been cut across.

Fig. 13. One of the forms assumed by some of the larvæ.

EXPLANATION OF PLATE V.

Fig. 14. Larva in the process of splitting into young Medusæ.

Fig. 15. Lower surface of one of the young Medusæ after its separation from a larva: *a*, one of the four bifid processes in the stomach; *c*, ocellus.

Fig. 16. Greatly enlarged view of one of the bifid processes in the stomach.

Fig. 17. Greatly enlarged view of one of the eight marginal rays or lobes: *a*, ocellus.

Fig. 18. Greatly enlarged view of the crystals in apex of ocellus.

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[1] Annals of Natural History, vol. v. p. 180.

Fig. 19. Five of the nucleated cells and several of the nuclei that enter so abundantly into the structure of the internal layer, as seen when a portion of this layer is detached.

Fig. 20. Small portion of a tentaculum, highly magnified, to exhibit the filiferous capsules adhering to its outer surface.

Fig. 21. Representation of *Actinia cylindrica* of the natural size.

Fig. 22. Oral surface of one of the tentacula.

In the examination of the more minute structures figured above, a one-eighth of an inch object-glass made by Powell and Leland, and a one-fourth of an inch object-glass by Smith and Beck, were employed.

ERRATA.

Page 5, last line, *for* of experiment, *read* of experimenting.
Page 122, footnote 4, *for* Chéim, read Chim.
Page 285, line 11, *dele* now.
Page 287, line 7, *dele* comma after the word pavement.
Page 518, line 12, *for* these, *read* those.
Page 552, footnote 2, *for* Clement, read Clémot.

EDINBURGH: T. CONSTABLE, PRINTER TO HER MAJESTY.